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Sociometry is concerned with networks of relationship among groups of people. If the group is very large, the work of tracing all the relationships becomes tedious, and the task of describing the resulting net precisely becomes impossible. Here the problem of such large sociometric nets is approached with probabilistic and statistical methods.

A STUDY OF A LARGE SOCIOGRAM

by Anatol Rapoport and William J. Horvath

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THE use of mathematical models to describe social structure has certain inherent advantages. A model which describes all the essential features of a specific social organization may have a more general applicability in that it may be applied directly or with minor changes to describe the structure of similar organizations. In fact, such differences as are found by fitting similar models to a group of organizations may permit a quantitative differentiation into separate classes of what might otherwise have been considered identical structures.

It is our purpose in this study to apply a mathematical description to a body of data obtained by querying the population of a particular junior high school in Ann Arbor, Michigan, as to their friendship preferences. It is not expected that the actual numbers and detailed structural properties obtained in this study will be duplicated in any other junior high school, but certainly some of the gross structural properties which were found in this situation should apply to other similar organizations; and the particular approach described here could serve as a prototype for other similar studies.

DEFINITION OF A SOCIOGRAM

A sociogram is a description of a population in terms of relations between pairs of people in that population. This relation may be "likes" (used in this study), "dislikes," "chooses as work companion," or any of a number of others. This relation may be bivalent, e.g., "present or absent" or "positive or negative", or it may be multivalent, e.g., "likes very much," "likes a little," "doesn't know," "dislikes," and "hates."

The set of values that the relation between A and B may take on is called the "*range*" of the function. The actual values are often expressed as numbers; 1 if the relation is present, 0 if it is absent, -1 if it is negative, -2 if it is strongly negative, etc. The *domain* of the function is the set of all ordered ($AB \neq BA$) pairs in the population. At least temporarily, A may like B while B dislikes A, so we must distinguish between the pair (A, B) and the pair (B, A).

If a relation is bivalent and it is the only one being considered, the sociogram can be represented either by a directed linear graph or as a matrix (α_{ij}), where the entry α_{ij} is the value of the relation which obtains between the ordered pair (i, j).

If the population is very large, the specification of the complete sociogram may be impractical; and even if such a description were available, the computational problems of treating linear graphs or matrices of such magnitude would be too formidable even for modern high-speed computers. The theoretician's interest, however, is seldom focused on a *particular* large sociogram (although particular small sociograms may sometimes be of great interest). Rather, the interesting features of large sociograms are revealed in their gross, typical properties. Thus one seeks to define classes of sociograms, or else to describe them by a few well-chosen parameters. It is perhaps natural to consider statistical parameters, since

one is interested in trends or averages, or distributions rather than particulars.

As for the taxonomy of large sociograms, this apparently involves problems of great complexity. It would seem offhand that a taxonomy of "nets" (the mathematical representations of sociograms) would arise naturally from the consideration of the statistical parameters, e.g., as a continuum of nets in the parameter space. But the statistical parameters themselves are singled out on the basis of taxonomic considerations, which have yet to be clarified. The nature of this methodological difficulty will become apparent in the course of our discussion.

The importance of an adequate theory of nets for quantitative sociology and social psychology is obvious. The social behavior of an individual is certainly strongly dependent on the behavior of others and perhaps most strongly on the behavior of *certain* others. The impacts upon an individual of the influence of other individuals follow the paths determined by the relevant sociometric net. These paths may be lines of authority, attraction, emotional involvement, etc. Also, the social behavior of the entire population is dependent on such patterns of relations, barring the trivial case when every member of the group is entirely and directly under the command of an outside authority, e.g., a platoon executing in unison the commands of a drill sergeant.

The spread of an attitude or of a piece of information through a large population is a case in point. There exist fairly sophisticated mathematical theories of contagion, in which many of the obvious parameters are included, e.g., infectivity, immunity, incubation lag, recovery or removal rates, etc. Missing from most (though not all) of these theories is a consideration of the contact structure of the population. By and large, in most theories of contagion, the population is assumed to be "well-mixed." That is to say, the probability of contagion in a given period of time between any two members of the population is taken as a parameter which may be a function of time (say the total duration of the contagion or the time since "infection" of the individuals concerned) but is the same for all pairs, implying that the probability of contact is independent of the pair.

To whatever extent this may be a fair approximation to a realistic theory of contagion, certainly an important next step is the consideration of cases where the probability of contact is not the same for every pair. This involves the consideration of the contact structure of the net.

The assumption of equiprobability of contact or well-mixedness is equivalent to the assumption of random contact structure. A random net can be defined by the process of its construction as follows:

From each individual or "node" let there be a choices or "axones" issuing.[1] Each of these axones settles on a target node within the population. Which particular node a particular axone settles on is determined by a chance device selecting with equal probability from the entire population. The resulting net is a "random net" with an "axone density" of a.

A net which is not random will be called a "biased" net. Obviously a random net is a special case of a biased net in which all the biases are zero. It is not easy, however, to define the various biases rigorously even though their nature can be intuitively grasped. For if the choices of the terminal points of the axones are not equiprobable, there are innumerable other ways in which they can be assigned. In particular, they can be assigned "absolutely" or "relationally." An absolute assignment is one where the probability assigned to a node of being the target of an axone depends only on the former node. A relational assignment is one where this probability depends on both the potential target and the origin of the axone.

A bias would operate "absolutely" if sociometric choices depended entirely on the personal characteristics of the individuals, e.g., on their "popularity." On the other hand, if a bias were the reflection of a "social distance" between the individual choosing and the individual chosen, one would have a "relational" bias.

[1] We will assume that a is equal for every member of our nets, but the concept of random nets may be generalized by allowing a to be random also.

Several different forms of bias have been investigated (Rapoport, 1951, 1953, 1956, 1957; Solomonoff & Rapoport, 1951) and one previous experiment dealing with the determination of bias (Dodd, Rainboth, & Nehnevajsa, unpublished) has been reported.

THE DATA COLLECTION FOR THE PRESENT STUDY

To develop further the techniques used in the previous investigations, data for the sociogram of a junior high school in the Ann Arbor area were collected shortly after the beginning of the 1960–61 school year. Each pupil in both schools was asked to write his name, age, grade, and home room number on a card and to fill in the blanks in the statements:

"1. My best friend in (name of school) Junior High School is_____
2. My second best friend. . ._____
. . . .
8. My eighth best friend. . . is_____."

Naturally some pupils were absent the first day and an attempt was made to cover them by returning a week later. Even so, there remained some absentees and some children who, though present, did not fill out their cards. All of our analyses so far show that these "absentees" are a random sample from the population and so no bias was introduced by this less-than-perfect data gathering.

Of the many quantitative properties which may be studied in such a large sociogram, we will report in this paper on just two, the popularity distribution, and the "connectivity" of the net. The first of these is a distributional property and the second a structural one. Other measures were applied as well and some of these may be reported in a later paper.

DISTRIBUTION OF POPULARITY

Tabulations were made of the number of individuals who received a given number of votes as first, second, third, etc., friend. This is shown in Table 1. According to this tabulation 380 persons received no votes as first friend, 248 received 1 vote as first friend, etc., 1 person receiving 10 votes as first friend.

TABLE 1
NUMBER OF PERSONS RECEIVING GIVEN NUMBER OF VOTES

Number of votes received	First friend	Second friend	Third friend	Fourth friend	Fifth friend	Sixth friend	Seventh friend	Eighth friend
0	380	399	413	413	407	417	418	439
1	284	257	263	265	257	252	264	244
2	136	145	112	116	133	122	118	115
3	42	33	38	35	41	50	35	44
4	11	19	22	18	11	12	17	8
5	5	3	10	8	10	2	3	6
6	0	1	0	3	0	2	4	3
7	0	1	1	1	0	2	0	0
8	0	1						
9	0							
10	1							
Unused ballots	98	101	113	120	119	129	147	173

The only consistent difference among the votes received for first, second, third, etc., friend as revealed by this table is the gradual increase in number of persons receiving zero votes in going from the first friend to eighth friend choices. This is largely accounted for by the increase in unmarked, missing, and mistaken ballots as the voting progressed from the first friend to the eighth friend. There were 98 such ballots in the first friend balloting and 173 in the eighth friend. Thus the increased number of persons receiving one and two votes as the first friend are transferred to the zero vote category in the eighth friend balloting by approximately the increased number of unused ballots.

The nature of the distribution function

A study of the mathematical form of the distribution of votes among the school population can give some insight into the process of choice used by the students in picking friends. For example, if everyone in school were equally popular and all students submitted ballots, then we would expect an average of one vote per person for the nth friend and the distribution of the number of zero, one, two, three, etc., votes would be given by the Poisson distribution with expected value 1; Ne^{-1}, $\frac{Ne^{-1}}{1!}$, $\frac{Ne^{-1}}{2!}$,

$\frac{Ne^{-1}}{3!}$, etc., where N is the total school population and e is 2.718. In Table 2 we have fitted Poisson distributions to the observed vote distributions for second and fourth friends using the expected values found from the data of votes per person of 0.882 and 0.860. Comparisons were also made for ballots for the other friends but these two sets of data typify the results. In all cases the Poisson distribution predicts a higher than observed value of one vote and lower values of zero and four, five, six and more votes. A Chi-square test shows that discrepancies of this magnitude are beyond the .05% level of chance fluctuations.

Greenwood and Yule (1920), in a study of accident statistics, have suggested a modification of the Poisson distribution to take into account the fact that not every person has the same expectation (of receiving a vote). The probability of receiving x votes is then given by

$$(1) \qquad p(x) = \int_0^\infty p(x, z)p(z)\, dz.$$

$p(x, z)$ is the Poisson distribution with expectation z, and $p(z)$ is the probability that a person chosen at random has an expectation lying between z and $z + dz$. For the function $p(z)$ they chose the Pearson Type III function

$$(2) \qquad p(z) = K\, e^{-\gamma z} z^{\alpha-1}$$

where α and γ are constants, and the normalization constant, $K = \frac{\gamma^\alpha}{\Gamma(\alpha)}$. Evaluation of the function $p(x)$ with the distribution function given in (2) leads to the negative binomial distribution in (3) below:

$$(3) \qquad p(x) = \binom{-\alpha}{x}\left(\frac{\gamma}{\gamma+1}\right)^\alpha\left(\frac{-1}{\gamma+1}\right)^x$$

The distribution (3) is sometimes called the Compound Poisson but since the Poisson distribution can be combined with other types of weighting functions, we shall call it the Greenwood-Yule distribution.

An entirely different derivation of (3) may be obtained from the theory of stochastic processes, where one assumes a time-dependent process such that the probability that at any given time, t, an event will occur in the interval $(t, t + dt)$ increases linearly with the number, x, of the events which have occurred in (o, t), and for a given value of x the probability decreases with increasing value of t. This particular contagious distribution is called a Polya process and has been used to describe the morbidity in epidemics. It is interesting that the identical distribution can result from two very different processes; Greenwood and Yule assume that the events are mutually independent, and that the intensities vary from individual to individual, while Polya assumes that the events are stochastically dependent, the occurrence of an event increasing the probability that further events will occur. Thus, a good agreement

TABLE 2

COMPARISON OF OBSERVED AND FITTED VALUES

Number of votes per person	Second Friend			Fourth Friend		
	Observed value	Poisson distribution	Greenwood-Yule distribution	Observed value	Poisson distribution	Greenwood-Yule distribution
0	399	356	399	413	363	420
1	257	314	267	265	313	253
2	145	138	122	116	134	114
3	33	40.5	46.6	35	38.5	45.9
4	19	8.90	16.2	18	8.25	17.2
5	3	1.54	5.3	8	1.45	6.2
6	1	.23	1.6	3	.18	2.18
7	1	.029	.49	1	.02	.75
8	1	.0032	.15	0	—	—

TABLE 3
CONSTANTS FOR THE GREENWOOD-YULE DISTRIBUTION

Friend	γ	α
1	4.45	3.95
2	3.18	2.80
3	2.41	2.09
4	2.35	2.02
5	3.59	3.09
6	2.91	2.47
7	2.99	2.48
8	2.73	2.18
Average all friends	3.08	2.64

between an observed distribution and the distribution (3) may be interpreted in two ways, and further analysis will be needed to determine which model offers the best explanation for the generation of the observed values.

The distribution (3) was tested against the same set of data shown in Table 2 for which the Poisson distribution gave a poor fit. The constants γ and α were evaluated using the observed means, $\bar{x}$, and variances, s^2, according to the expressions[2]

$$(4) \qquad \gamma = \frac{\bar{x}}{s^2 - \bar{x}} \qquad \alpha = \gamma\bar{x}$$

The fitted values were then calculated and the results are presented in Table 2. The fit to the observed value is rather close. The Chi-square test shows agreement with the observed value at the 20% level for the second friend data and 60% level for the fourth friend. The fitted values for the other friends were all within this range of closeness.

The values of α and γ calculated from the relations (4) above have been tabulated in Table 3. Except for the fact that the values for the first friend are slightly higher than for the other choices, there is no noticeable trend in the values. The probability density function, $p(z)$, of equation (2) has been plotted in Fig. 1 for the first, third, fifth, and seventh friend choices. The curves are seen to be extremely close together with no significant trend in going from the higher to the lower order of friends. The close agreement in the "Popularity Intensity Functions" among the different orders of friends suggests that an average curve derived from the data can be used to describe the situation for any order of friend. One might also infer from this that if a certain person has a high intrinsic "popularity intensity" for first friend, he would have this same value for second, third, etc., friend.

If we make the assumption that the same intensity function applies to all order of friends, then it is possible to calculate the joint probabilities of being chosen x_1 times as first friend and x_2 times as second friend. This will be given by

$$(5) \qquad p(x_2/x_1) = \int_0^\infty p(x_2, z)p(x_1, z)p(z)\, dz$$

substituting the Poisson functions and the

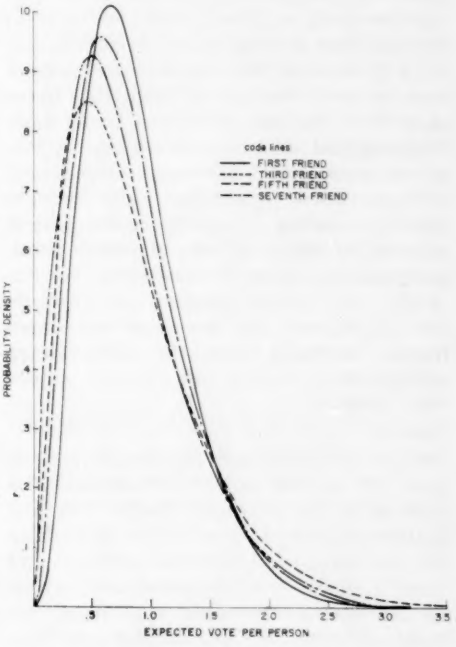

FIG. 1. Popularity intensity functions obtained from best fit of Greenwood-Yule function to observed popularity distribution.

[2] For approximate methods of fitting the negative binomial see Hald (1955, p. 727–731).

TABLE 4

COMPARISON OF OBSERVED AND CALCULATED NUMBER OF PERSONS RECEIVING GIVEN NUMBER OF VOTES AS SECOND FRIEND AFTER RECEIVING A STATED NUMBER OF VOTES AS FIRST FRIEND

First friend	0		1		2		3		4		5	
Second friend	Obs.	Calc.	Obs.	Calc.	Obs.	Calc.	Obs.	Calc.	Obs.	Calc.	Obs.	Calc.
0	193	197	131	121	53	46.5	16	14.3	4	3.9	1	1.0
1	114	121	91	93	37	43.0	12	15.5	2	4.8	1	1.3
2	56	46.5	46	43.0	30	23.1	8	9.5	3	3.3	2	1.0
3	10	14.3	11	15.5	7	9.5	4	6.6	1	3.4	0	1.4
4	5	3.9	5	4.8	7	3.3	0	3.4	1	2.9	1	1.8
5	2	1.0	0	1.3	0	1.0	1	1.4	0	1.8	0	1.8

Intensity function from (2) we get

$$p(x_2/x_1) = \frac{\gamma^{\alpha}}{(2+\gamma)^{\alpha+x_1+x_2}} \cdot \frac{\Gamma(\alpha + x_1 + x_2)}{\Gamma(\alpha)x_1!\, x_2!} \tag{6}$$

Table 4 compares the values calculated from this formula for the number of persons receiving a given number of votes as second friend after having received 0, 1, 2, 3, 4, or 5 votes as first friend. The values of α and γ were obtained by fitting the Greenwood-Yule function to the first friend data. The observed values are shown side by side in the same table. Although the observed and calculated values are quite close, a model assuming complete independence between the choice of first and second friends gives almost as good agreement. For example, 380 persons received no votes as first friend and 399 no votes as second friend. Assuming complete independence and random choice we would expect $\frac{380 \times 399}{859} = 179$ to receive zero votes for first and second friend. This is not quite as good a fit to 193 as the 197 calculated by formula (6) but is actually fairly close. The number expected to receive 1 vote for both first and second friend on the assumption of random choice is 88 compared with 93 for the correlated choice model. Again the observed value, 91, falls between the two calculated values.

Apparently the observed Intensity Function produces only a minor perturbation in the random choice model if only the first and second conditional choice probabilities are considered. However, we can also calculate the distribution of the number of votes

TABLE 5

NUMBER OF PERSONS RECEIVING GIVEN NUMBER OF VOTES FOR ALL EIGHT FRIENDS

Number of votes	Observed number of persons receiving votes	Calculated number
0	26	29.7
1	43	56.5
2	61	74.5
3	82	83.2
4	95	85.0
5	83	81.8
6	93	75.4
7	68	67.5
8	49	58.9
9	63	50.5
10	42	42.6
11	32	35.5
12	30	29.2
13	17	23.8
14	12	19.3
15	19	15.5
16	9	12.4
17	5	9.9
18	6	7.8
19	7	6.2
20	1	4.8
21	5	3.8
22	2	2.9
23	2	2.3
24	2	1.8
25	1	1.4
26	1	1 0
27	1	.8
28	1	.6
29	1	.5

received for all eight friends on the assumption of an average Intensity Function which is identical for all friends. The convolution integral in this case can be reduced to the following rather simple expression:

$$P_8(n) = \left(\frac{\gamma}{8+\gamma}\right)^{\alpha}\left(\frac{8}{8+\gamma}\right)^{n} \cdot \frac{\Gamma(\alpha+n)}{n!\Gamma(\alpha)} \tag{7}$$

where n is the number of votes received for all eight friends, α and γ are the average values from Table 3, and $P_8(n)$ gives the probability that a person received n votes for all eight friends.

The observed and calculated values have been tabulated in Table 5. The agreement between formula (7) and the observed values is excellent. A Chi-square test gives a probability of 0.55 that the observed variations are due to chance. The independent choice model would predict only two persons to receive zero votes for all eight friends compared with the observed value of 26 and the calculated value of 30 from formula (7).

The conclusion to be drawn from this analysis is that the observed distribution of votes among the school population can be satisfactorily explained by the Compound Poisson distribution of Greenwood and Yule or alternatively as a Polya "Contagion Process." Whatever the mechanism leading to this result, it may be viewed as though each person had a characteristic popularity intensity which is the same for all ballots from the first to eighth friend.

CONNECTIVITY

Consider a net (random or biased) with axone density a (small) and population N (large). Arbitrarily select a small fraction of nodes p_0. From these nodes ap_0N axones issue, which terminate on some set of nodes. Among these target nodes, some may be members of the starting set. Others will be newly contacted. Call the fraction of the population represented by the newly contacted nodes p_1. Continue the process, calling each newly contacted fraction p_2, p_3, $\cdots$ p_t $\cdots$. Thus the nodes represented by p_t are those t times removed from the starters. Eventually some p_t will be zero because all of the nodes contacted on the previous remove will be among the $p_j (j = 0, 1 \cdots t-1)$, and no new ones will have been contacted. At this point our tracing procedure is ended.

We shall be interested in the expected values of p_t and of the derived variables. $X = \sum_{j=0}^{t} p_j$ and its asymptotic value, X_∞, as functions of a and of the bias parameters.

The expected values of p_t in a random net have been previously derived by one of us (Rapoport, 1951). They are given by the following iteration formula

$$p_{t+1} = (1 - X_t)(1 - e^{-ap_t}). \tag{8}$$

It has also been shown (Solomonoff & Rapoport, 1951) that the expected cumulated fraction of the population so traced satisfies the transcendental equation

$$X_\infty = 1 - (1 - p_0)e^{-a}X_\infty. \tag{9}$$

If the number of possible targets for each axone is reduced from N (the total population) to q (the size of an "acquaintance circle"), formulas (8) and (9) still hold even if $q \ll N$, so long as $q \gg 1$ and provided the intersection of the acquaintance circles of two individuals who are themselves acquainted has the same expected number of individuals in common as the intersection of two arbitrarily selected acquaintance circles.

On the other hand, if we assume that the acquaintance circles of two acquainted individuals are identical, this amounts to the assumption that the entire population "falls apart" into a set of mutually exclusive cliques of size q. If *within* the cliques, targets are arbitrarily chosen, we have several small random nets instead of a single large one. In a previous treatment of the problem (Rapoport, 1953), bias was defined in such a way that the "overlap" between the acquaintance circles of acquainted individuals was denoted by a parameter θ, $0 \leqslant \theta \leqslant 1$. The case $\theta = 0$ corresponded to the random net, while $\theta = 1$ was a reflection of a very tight bias

in which friends of friends were very likely (but not certain) to be friends. The iteration formula for the biased case will then be given by an expression identical to (8) except that an "apparent axone density" α replaces the a in (8). After the first remove α is given by the approximate expression

$$\alpha \cong 1 - e^{-a\theta} + (1 - \theta)a. \tag{10}$$

It is substantially independent of t and varies from $1 - e^{-a}$ for $\theta = 1$ (strongest bias) to a for $\theta = 0$ (random net).

"TRACING" THE SOCIOGRAM

The information obtained from the ballots returned by the students was coded and tabulated on punched paper tape so that it could be stored on the drum of an LGP 30 computer. A number of different programs were prepared for the analysis of the net structure. The particular data reported on in this paper were obtained by starting with a random sample of nine ballots and tracing two choices at a time, for example the nth and $(n + 1)$th friends. The number of new nth and $(n + 1)$th friends contacted on each remove was determined until the chain ended either on a person previously contacted or a person who did not return a ballot. The whole procedure was then repeated on another random sample of nine. A total of at least 30 such random samplings were made and the chain of nth and $(n + 1)$th friends was traced forward each time until no further new contacts were made and the chain terminated.

Since no behavioral process is involved in such a tracing, obviously explanations based on motivation and the like should not enter into the theory. Therefore we expect that if the theory of biased nets leading to equation (10) is correct, then the equation involving θ as a constant should adequately describe the tracing.

If we confined ourselves to a tracing over the first and second friends only, we obtained an average a of about 1.75 instead of 2.0, because of the axones "lost" in the blanks. (Absentees appeared in our population as cards with no blanks filled.) This reduction

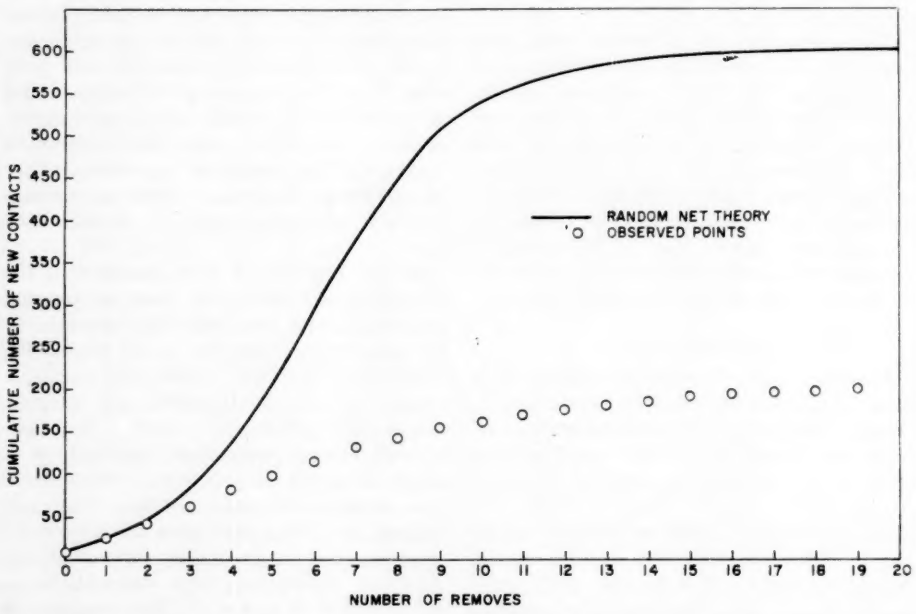

FIG. 2. Cumulative Number of New Contacts as a Function of the Number of Removes Compared with Random Net Theory. First and Second Friends.

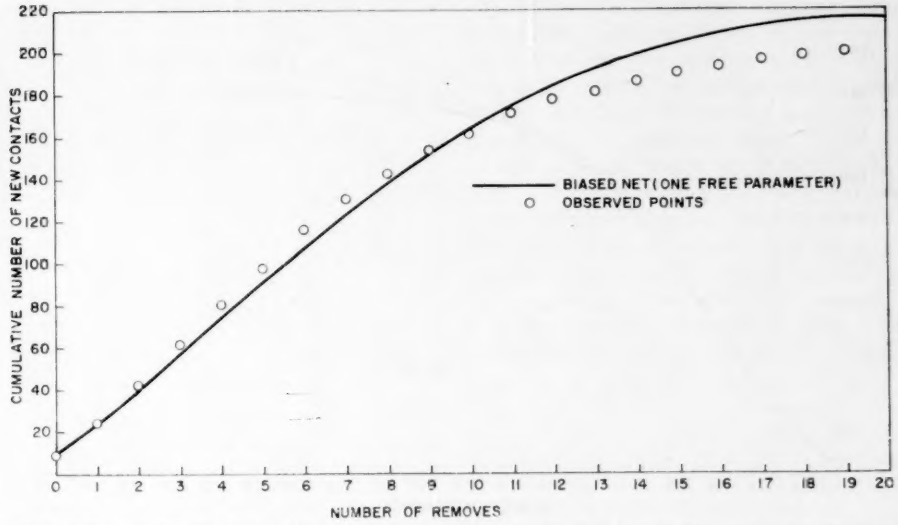

FIG. 3. Cumulative Number of New Contacts Compared with Biased Net Theory $\theta = 0.8$. First and Second Friends.

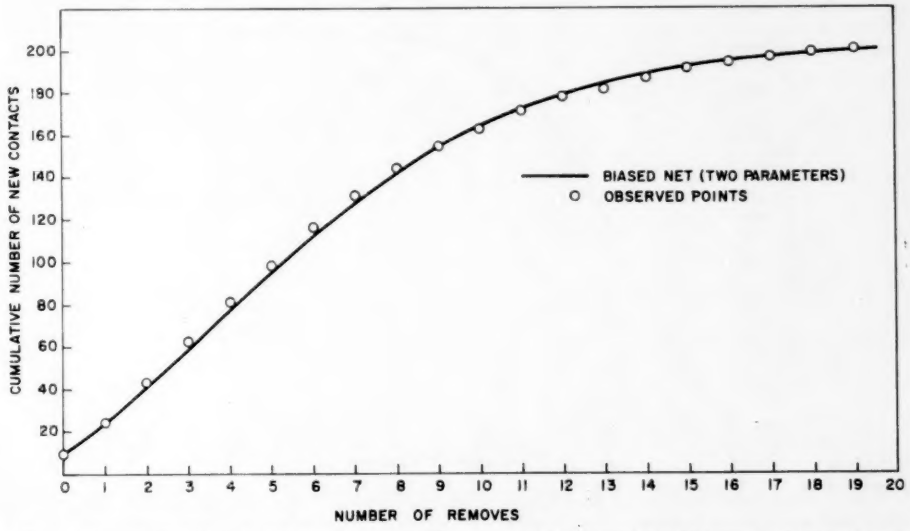

FIG. 4. Cumulative Number of New Contacts Compared with Biased Net Theory $\theta = 0.77$ and $N^* = 601$. First and Second Friends.

of the "intended" a to an "effective" a makes for no theoretical difficulty since, as we have noted, only the expected number of axones appears as a variable in the successive steps of our tracing.

Thus we have to deal with the following parameters: N, the total population; a, the *effective* axone density, to be estimated by comparing p_0 and p_1 via equation (8); and θ, to be estimated so as to give the best fit to the remaining curve.

Fig. 2 shows the comparison of the data

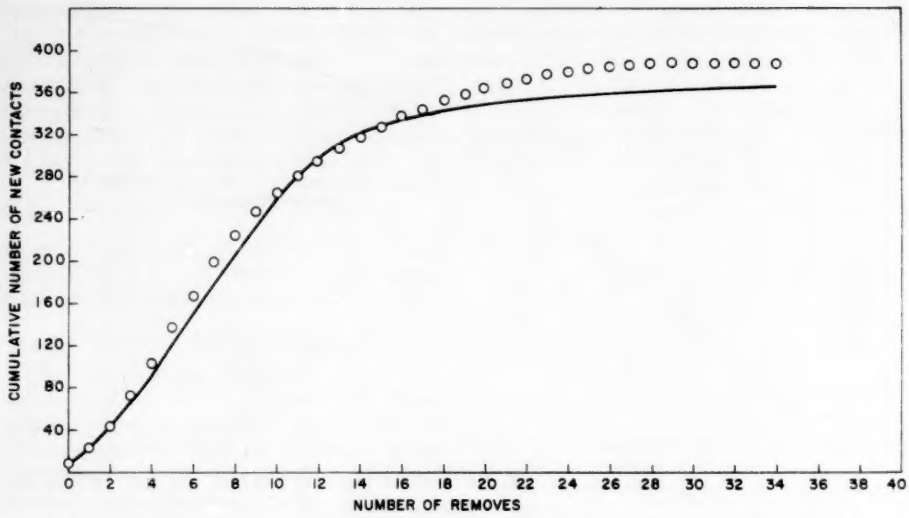

FIG. 5. Cumulative Number of New Contacts Compared with Biased Net Theory $\theta = .67$ and $N^* = 861$. Second and Third Friends.

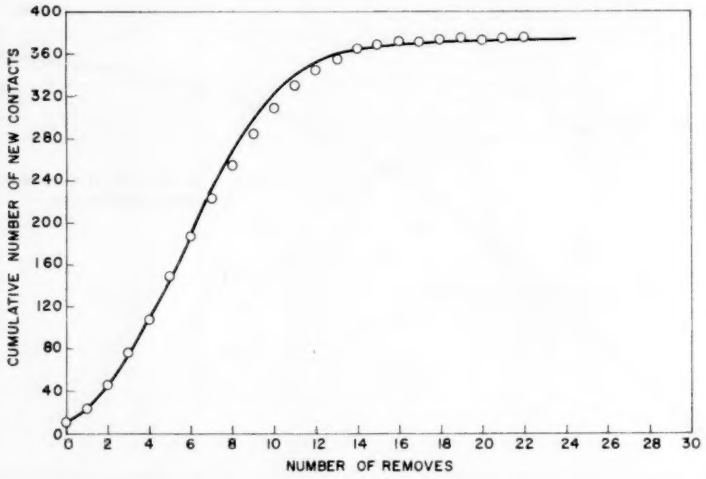

FIG. 6. Cumulative Number of New Contacts Compared with Biased Net Theory $\theta = .50$ and $N^* = 620$. Third and Fourth Friends.

with the null hypothesis, namely that the sociometric choice pattern constitutes a random net. There is little doubt that the hypothesis should be rejected.

Fig. 3 shows the same comparison under the assumption of a bias given by $\theta = 0.8$ (hence $\alpha(t) = 1.15$ for $t \geq 1$). There is a marked improvement in the fit, but still a sizeable discrepancy between the theoretically predicted and the observed curves for X_t.

It appears, therefore, that "cliquishness," as measured by a single parameter, does not entirely account for the discrepancy between

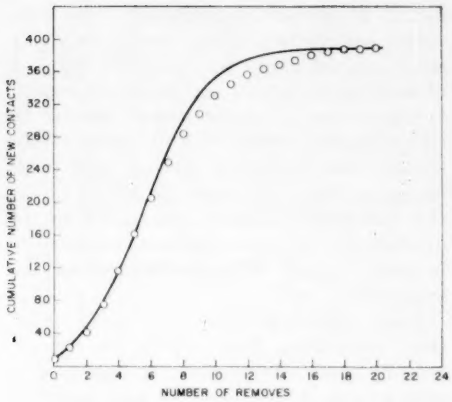

FIG. 7. Cumulative number of new contacts compared with biased net theory $\theta = .40$ and $N^* = 631$. Fourth and fifth friends.

implied by our acquaintance circle overlap parameter θ. We can, however, introduce a bias governing the *absolute* assignment of axones to targets, i.e., a "popularity" bias, which, one feels intuitively, ought to be independent of relational biases.

The question before us is how such a bias would be reflected in the *tracing* formula. Again a lack of rigorous theory compels us to venture a guess, namely, that the introduction of a popularity bias is tantamount to a reduction of the "effective" population, i.e., the value of N which is implicit in the calculation of the p_j and x_j. This interpretation is obvious in the special extreme case where the entire population consists of two classes, the "popular" and the "unpopular," meaning that the unpopular

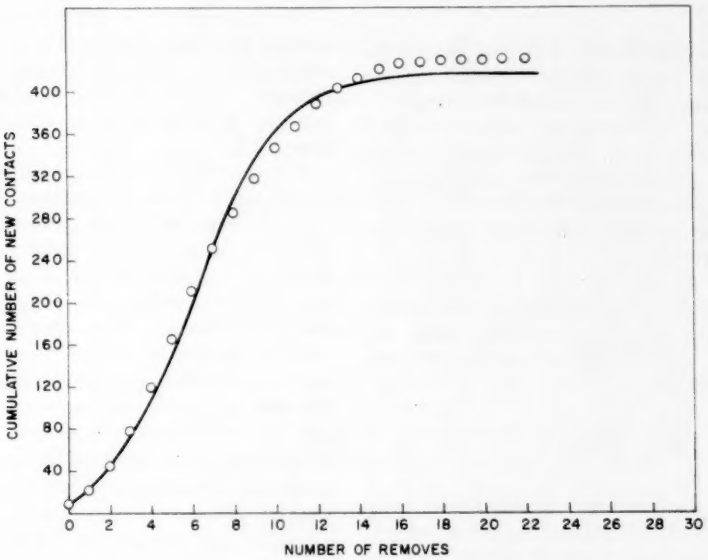

FIG. 8. Cumulative Number of New Contacts Compared with Biased Net Theory $\theta = .30$ and $N^* = 700$. Seventh and Eighth Friends.

the data obtained from average tracings and the expected tracing curve for a random net. The search for a second parameter seems indicated. But we cannot introduce a second *relational* parameter without risking a contradiction, for we have no way of knowing what relational biases are already

receive no sociometric choices, so that they all go to the popular.

Less extreme forms of the popularity bias should have a similar effect, namely a reduction of the "effective" population as it is reflected in the tracings. Introducing now $N^* = 601$ instead of $N = 861$ to calculate

TABLE 6
MEASURED VALUES OF CONSTANTS IN BIASED NET THEORY

Rank order of friends	a	α	θ	N*
1 and 2	1.78	1.15	.77	601
2 and 3	1.75	1.26	.67	861[a]
3 and 4	1.72	1.44	.50	620
4 and 5	1.73	1.53	.40	631
7 and 8	1.62	1.50	.30	700

[a] This value seems anomalous. It appears to indicate that popularity bias does not operate in the tracings of second and third friends. The value 861 was chosen as giving the best fit under the restriction $N^* \leqq N$ and this fit is still the worst among the tracings. The remaining values of N* increase with numerical rank order suggesting a weakening of the popularity bias. This apparently is not the case since direct examination of the distributions of sociometric choice show no such effect.

the p_j from the observed P_j, we obtain the theoretical curve for the average tracing through friends 1 and 2 as shown in Fig. 4.

We have now obtained the best possible fit to the data on an average tracing through first and second friends by adjusting two free parameters, α and N^*. There are, however, some restraints on both parameters. The parameter α is determined by θ and a (cf. equation 10). Of these, θ lies between 0 and 1, and a can be empirically determined by counting failures to name friends of the respective rank orders. It can also be computed from

$$a = \frac{1}{p_0} \log \frac{1 - x_0}{1 - x_1}. \tag{11}$$

The two quantities agree closely throughout.

The "effective" population N^* is subject to the restriction $N^* < N$ ($N = 861$), by our argument above concerning the nature of the popularity bias. Aside from these restrictions, θ and N^* (hence α and N^*) are free parameters. Curves as smooth as the cumulative tracing curves can probably be comfortably fitted by two almost free parameters regardless of the underlying model, so long as the mathematical function to be fitted has the required properties (i.e., almost a constant slope initially and horizontal asymptote). Thus the fits in themselves do not imply a strong corroboration of our theory. Intuitively we would expect, however, that θ should monotonically decrease as the numerical rank order of the friends through which tracings are made increases. This is because one would expect the friendship relations, and therefore the overlap bias of the acquaintance circles, to become less tight with increasing numerical rank order.

Since we have interpreted N^* as an "effective" population from which choices are actually made, we would not expect it to vary greatly with increasing rank order of the friendship, since, as we have previously shown, the popularity intensity function does not change significantly in going from the first to the eighth friend.

The data which are exhibited in Figs. 5–8 show comparisons between theoretical curves and data in tracings through successive consecutive pairs of friends. Table 6 summarizes the behavior of the parameters a, α, θ, and N^*.

We see that a [calculated from (11)] remains almost exactly constant in the first four tracings and drops somewhat in the last. This drop is exactly accounted for by a direct count of "failures to name." The parameter α increases in the first four tracings but remains about the same in the 7–8 tracing as in 4–5. However, since a decreases, θ also decreases. The monotone decrease of θ with rank order makes reasonable our interpretation of that parameter as a measure of tightness of acquaintance circles and corroborates our hypothesis concerning the relaxation of this tightness with increasing rank order. The effective population N^*, on the other hand, tends to increase steadily somewhat with rank order of the friendship choice, except for one anomalous value in the 2–3 tracing. Since we would expect no increase because of the constancy of the popularity bias, the significance, if any, of this variation cannot be explained in terms of our present theoretical framework.

REFERENCES

Dodd, S. C., Rainboth, E. D., & Nehnevajsa, J. *Revere studies on interaction.* Washington Public Opinion Laboratory, unpublished.

Greenwood, M., & Yule, G. U. An inquiry into the nature of frequency distributions representative of multiple happenings. *J. Roy. Stat. Soc.*, 1920, 83, 255–279.

Hald, A. *Statistical theory with engineering applications.* New York: John Wiley, 1955.

Rapoport, A. Nets with distance bias. *Bull. Math. Biophysics*, 1951, 13, 85–91.

Rapoport, A. Spread of information through a population with sociostructural bias. *Bull. Math. Biophysics*, 1953, 15, 523–543.

Rapoport, A. The diffusion problem in mass behavior. *General Systems*, 1956, 1, 48–55.

Rapoport, A. Contribution to the theory of random and biased nets. *Bull. Math. Biophysics*, 1957, 19, 257–277.

Solomonoff, R., & Rapoport, A. Connectivity of random nets. *Bull. Math. Biophysics*, 1951, 13, 107–117.

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There are two equal and eternal ways of looking at this twilight world of ours: we may see it as the twilight of evening or the twilight of morning; we may think of anything down to a fallen acorn as a descendant or as an ancestor. There are times when we are almost crushed, not so much with the load of evil as with the load of the goodness of humanity, when we feel that we are nothing but the inheritors of an ancient splendor. But there are other times when everything seems primitive, when the ancient stars are only sparks blown from a boy's bonfire, when the whole earth seems so young and experimental that even the white hair of the aged, in the fine Biblical phrase, is like almond trees that blossom, like the white hawthorn grown in May. That it is good for a man to realize that he is "the heir of all the ages" is pretty commonly admitted; it is a less popular but equally important point that it is good for him sometimes to realize that he is not only an ancestor, but an ancestor of primal antiquity; it is good for him to wonder whether he is not a hero, and to experience ennobling doubts as to whether he is not a solar myth.

G. K. CHESTERTON, *A Defence of Nonsense*

The following study examines the effect of the individual's place in the social structure on his mental health. The more restrictive his social environment, the greater is his frustration, and consequently, the greater the likelihood of mental illness.

MULTIPLE GROUP MEMBERSHIP AND SCHIZOPHRENIA

by Robert J. Kleiner and Jacob Tuckman

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In previous studies on the relationship between status and pattern of mental disorder (Kleiner, Tuckman, & Lavell, 1959, 1960), the incidence of paranoid and other schizophrenic reactions was found to be higher in a low- than in a high-status group; also, the onset of mental illness, as shown by first admission to a psychiatric hospital or a diagnostic facility, occurred at an earlier age in the low-status group. The higher incidence of schizophrenic reactions in the low-status group was attributed to the triggering effect of a controlling or hostile environment; the earlier onset of mental illness, to the earlier impairment of frustration tolerance stemming from the pervasive restrictions of the social environment.

The differences between high- and low-status groups with respect to the amount of schizophrenia and age at the time of onset of mental illness were in the predicted direction when status was based on racial membership, i.e., white (high status) or Negro (low status); on religious affiliation, i.e., Protestant (high) or Catholic (low); and on subgroup membership within the Protestant denomination, i.e., Episcopalian or Presbyterian (high), or Methodist or Baptist (low).

Obviously, the three studies suggest that similar relationships would be found in any examination of status difference in group membership. For example, with respect to sex, men may be designated as high status and women as low. An analysis of the sample used in the studies dealing with racial membership and religious affiliation referred to above showed a higher proportion of schizophrenia for women than for men, at 43% and 39%, respectively. National statistics on first admissions to all types of mental institutions in the United States for 1950 and 1951 indicated an even greater sex difference in the predicted direction: of all first admissions, 26% were for women schizophrenics, and 19% for men (U. S. Department of Health, Education, and Welfare, 1954). This sex difference was also evident in each type of mental institution. For private hospitals, the percentages of schizophrenics among first admissions were 22% for women and 15% for men; for state hospitals, the corresponding percentages were 29% and 20%; for county and city hospitals combined, 17% and 12%, respectively.

The onset of mental illness at an earlier age in women, the low-status group, is not supported by data from the sample referred to above, but is evident from national statistics based on more than 280,000 psychiatric first admissions (U. S. Department of Health, Education, and Welfare, 1954). For psychotic disorders, the median age at first admission was 48.4 years for women and 51.4 years for men; for psychoneuroses, the corresponding median ages were 37.2 and 38.9 years, respectively; for first admissions without psychosis, 37.6 and 41.0 years, respectively.

In a previous study (Kleiner et al., 1959), a tentative model was suggested for ordering multiple status-group memberships according to their susceptibility to schizophrenia, based on the number of low-status memberships as well as the differential contribution

of each low status. Using racial membership and religious affiliation, it was fairly simple to order the four possible combinations—white Protestants, white Catholics, Negro Protestants, and Negro Catholics. The white Protestants and Negro Catholics presented no ordering problem, since the latter occupied low-status positions in two memberships and the former, in none. Since it seemed reasonable to assume that the restrictions of the social environment were more pervasive for race than for religion, white Catholics were placed above Negro Protestants in the hierarchy. Obviously, as the number of group memberships increases, the task of ordering becomes more and more difficult. The purpose of this paper, therefore, is (1) to offer a procedure, based on empirical data with respect to schizophrenia, for ordering any number of group memberships into a status hierarchy, provided each group membership can be dichotomized into high and low status; and (2) to illustrate the application of the procedure to three group memberships—sex, race, and religion.[1]

SAMPLE AND PROCEDURE

The sample consisted of 2,496 first admissions that could be classified within the three group memberships studied. The sample was distributed as follows: for race, 1,682 whites and 814 Negroes; for religion, 1,457 Protestants and 1,039 Catholics; and for sex, 1,230 men and 1,266 women. The distribution of the mental disorders showed 38% schizophrenic reactions and 62% other disorders. The sample was drawn from a total sample of 3,004 first admissions from the city of Philadelphia to state mental hospitals in Pennsylvania for the five-year period June 1, 1951, to May 31, 1956.

The simplest way of ordering groups according to the incidence of schizophrenia is to take into account the number of low-status positions occupied by the individual. The procedure assumes that as the number of low-status positions increases, frustration increases, resulting in an increasing incidence of schizophrenia. The ordering of groups according to the number of low-status positions may be summarized in Formula 1.

$$S_{(A_l+B_l+n_l)} > S_{(A_l+B_l+n_l-1)}$$

where S = relative incidence of schizophrenia
A_l = low status of group membership A
B_l = low status of group membership B
and n_l = the number of low-status memberships.

The data obtained by the application of Formula 1 to the sample described above supported the hypothesized relation between the number of low-status positions and schizophrenia. The incidence of schizophrenia was lowest among those occupying no low-status positions (white Protestant men, 29%); higher among those occupying one low-status position (Negroes, Catholics, or women; the average for the three groups was 34%); still higher for those occupying two low-status positions (Negro Catholics, Catholic women, or Negro women, an average of 58%); and highest among those occupying three low-status positions (Negro Catholic women, 78%).

A more detailed analysis comparing each combination of group memberships with every combination having fewer low-status positions also supported the hypothesis. Of the 22 comparisons, 20 showed that the cumulative effect was in the predicted direction (significant at the $< .01$ level). The two not in the predicted direction showed more schizophrenia among white Protestant men than among white Protestant women and more schizophrenia among Negro Protestant men than among white Catholic women.

It should be possible to order the eight possible combinations of race, religion, and sex into a single status hierarchy. While the application of Formula 1 makes it possible to order combinations by the number of low-status positions, it does not permit ordering two or more combinations having the same number of low-status positions but differing in composition, e.g., white Catholic

[1] The subgroup membership in the Protestant denomination referred to earlier was not utilized because the sample had been drawn from a different psychiatric population.

women and Negro Protestant women. A more productive method for ordering groups considers simultaneously the cumulative effect and the differential contribution of each type of group membership, and also controls for the interaction of different combinations of group memberships.

Formula 2 summarizes this procedure:

$$Wt_A \times Wt_B \times x \cdots Wt_n = \frac{\sum_{2^{n-1}} \left[\left(\frac{S_{A_l}}{S_{A_h}} \right) N_A \right] x \sum_{2^{n-1}} \left[\left(\frac{S_{B_l}}{S_{B_h}} \right) N_B \right] x \cdots \sum_{2^{n-1}} \left[\left(\frac{S_{n_l}}{S_{n_h}} \right) N_n \right]}{N_T}$$

where

Wt_A = the contribution of status difference in group membership A

S = the relative incidence of schizophrenia

A_l = the low status of group membership A

A_h = the high status of group membership A

n = the number of group memberships being controlled

N_n = number of schizophrenic patients in each type of group membership

N_T = total number of schizophrenic patients in the sample

Wt_B = the contribution of status difference in group membership B

and

Wt_n = the contribution of status difference in the last group membership

To determine the contribution of status difference in group membership A, e.g., race, it is necessary to compute the average effect of race in each of the four possible combinations of religious affiliation and sex, i.e., Protestant men, Protestant women, Catholic men, and Catholic women. The application of Formula 2 to the pertinent data in Table 1 showed the relative contribution of race to schizophrenia to be:

$$Wt_A = \frac{\left(\frac{50\%}{29\%}\right) 301 + \left(\frac{60\%}{22\%}\right) 203 + \left(\frac{65\%}{37\%}\right) 224 + \left(\frac{78\%}{43\%}\right) 213}{4} = 1.99.$$

Using the same procedure, the average contribution was 1.48 for religion and 1.10 for sex. Substituting the average contribution (Wt) of each membership in Formula 2, it is possible to obtain the relative weight of any pattern of group membership. For example, the weight for white Protestant men would be $1.00 \times 1.00 \times 1.00$, or 1.00; for Negro Catholic women, the weight would be $1.99 \times 1.48 \times 1.10$, or 3.24. The

TABLE 1

RELATIVE CONTRIBUTION OF RACE, RELIGION, AND SEX TO SCHIZOPHRENIA*

	N	Ratio of Negro/White		N	Ratio of Catholic/Protestants		N	Ratio of Women/Men
Protestant Men	301	1.72	White Men	291	1.27	White Protestants	180	.76
Protestant Women	203	2.72	White Women	276	1.95	White Catholics	387	1.16
Catholic Men	224	1.76	Negro Men	234	1.30	Negro Protestants	324	1.20
Catholic Women	213	1.81	Negro Women	140	1.30	Negro Catholics	50	1.20
Average Weight	941	1.99		941	1.48		941	1.10

* Observed percentages of schizophrenia in the sample were:

White Protestant Men	= 29	Negro Protestant Men	= 50
White Protestant Women	= 22	Negro Protestant Women	= 60
White Catholic Men	= 37	Negro Catholic Men	= 65
White Catholic Women	= 43	Negro Catholic Women	= 78

hierarchy established by the weighting system is shown in Table 2. The rank-order correlation between the numerical weights and the observed incidence of schizophrenia in the sample is .98, significant at the $< .01$ level.

DISCUSSION AND CONCLUSIONS

A procedure has been offered for ordering combinations of group membership into a status hierarchy, provided each group membership can be dichotomized into high- and low-status positions. The procedure considers simultaneously the number of low statuses and the relative contribution of each membership to the incidence of schizophrenia. The status hierarchy reflects differences in frustration imposed by the social environment.

The ultimate validity of the status hierarchy depends on the importance of such factors as the differential utilization of private and public facilities by these groups, which may be a function of attitudes toward psychiatry, attitudes toward public facilities, or knowledge of available facilities. While definitive research is necessary, available data do suggest that the observed differences between the groups in the status hierarchy cannot be explained by differential use of facilities. National figures show that the incidence of schizophrenia is higher for women than for men in public mental hospitals (U. S. Department of Health, Education, and Welfare, 1954). A study of first admissions to private mental hospitals in the Philadelphia area showed only 9% with a diagnosis of schizophrenia (Kleiner, 1958). Assuming that whites, the high-status group, contributed 100%, and Negroes, the low-status group, 0% of the first admissions to private hospitals, combined public and private first admissions serve only to accentuate the differences between the two groups. Hollingshead and Redlich (1958) have shown less acceptance of psychiatry by low-status groups, which also suggests that the observed differences with respect to schizophrenia may be an underestimation of true differences.

TABLE 2

THE HIERARCHY OF PATTERNS OF GROUP MEMBERSHIP BASED ON WEIGHTED VALUES

Weighted Values	Pattern of Membership	Observed Incidence of Schizophrenia (%)
1.00	White Protestant Men	29
1.10	White Protestant Women	22
1.48	White Catholic Men	37
1.63	White Catholic Women	43
1.99	Negro Protestant Men	50
2.19	Negro Protestant Women	60
2.95	Negro Catholic Men	65
3.24	Negro Catholic Women	78

Demonstrating a correlation between independent measures of status and the status hierarchy would be of more value in establishing the validity of the proposed status hierarchy than further study of differential utilization of public and private facilities. It is also important to determine whether prevalence data on the nonhospitalized mentally ill in the population support the status hierarchy based on hospitalized patients. The Midtown Project, a study of prevalence in a section of New York City, tended to show no differences in rates of mental illness between whites and Negroes, men and women, and Protestants and Catholics, when economic status was controlled.[2] The findings would support the status hierarchy because the differences in hospitalization rates among the several groups would then represent true differences. On the other hand, the finding of more mental illness among whites than Negroes in a community study in Baltimore (Pasamanick, Roberts, Lemkau, & Krueger, 1959) does not support the status hierarchy. However, both studies suffer from important limitations: extremely high estimates of mental illness and broad criteria of mental illness in the former study, and too few cases in the latter. More prevalence studies of group differences are clearly needed.

A procedure has been offered, based on empirical data with respect to schizophrenia, for ordering any group or combination of

[2] Findings reported by Dr. Thomas Langner of the Midtown Project Staff in a personal communication.

group memberships into a status hierarchy, provided each group membership can be dichotomized into high- and low-status positions. Three group memberships, sex, race, and religion, were used to demonstrate the procedure which takes into account the number of low-status positions and the relative contribution of each, at the same time controlling for the interaction of different combinations of group memberships. The basic assumption is that the incidence of schizophrenia is a function of frustration stemming from restrictions of the social environment. The amount of frustration is a function of the cumulative effect of low statuses and the differential effect of the several memberships.

The data have implications for research and clinical practice. For the experimenter, they point up the need to identify factors that contribute to the differential effect of various group memberships, alone or in combination. The differential effect will be better understood by studying factors such as the importance for the individual of the group membership, identification with the group, or anchorage in it; the amount of acceptance or rejection by the social environment; and the interaction of these psychological and social factors. Additional factors might include ease of identification of the individual as a group member, ease of denial or affirmation of membership by the individual, his desire to deny or affirm membership and under what conditions, etc.

The clinician has a more difficult problem. While he also needs to be concerned with the kind of problems outlined above, his primary focus is the patient himself. "Awareness of the environment" is not enough. He will be more effective if he focuses more sharply on the effects of such problems on the individual and relates them to differential diagnosis and treatment.

REFERENCES

Hollingshead, A. B., & Redlich, F. C. *Social class and mental illness*. New York: John Wiley, 1958.

Kleiner, R. J. A comparative study of state and private hospitals. Unpublished report, Pennsylvania Department of Public Welfare, 1958.

Kleiner, R. J., Tuckman, J., & Lavell, M. Mental disorder and status based on protestant subgroup membership. *J. Soc. Psychol.*, in press.

Kleiner, R. J., Tuckman, J., & Lavell, M. Mental disorder and status based on religious affiliation. *Human Relations*, 1959, 12, 273–276.

Kleiner, R. J., Tuckman, J., & Lavell, M. Mental disorder and status based on race. *Psychiatry*, 1960, 23, 271–274.

Pasamanick, B., Roberts, D. W., Lemkau, P. W., & Krueger, B. K. A survey of mental disease in an urban population: prevalence by race and income. In B. Pasamanick (Ed.), *Epidemiology of mental disorder*. Washington: Amer. Ass. for the Advancement of Sci., 1959, Publication No. 60, 183–203.

U. S. Department of Health, Education and Welfare, Public Health Service, National Institutes of Health. *Patients in mental institutions, 1950 and 1951*. Washington: U. S. Government Printing Office, 1954.

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Where there is much desire to learn, there of necessity will be much arguing, much writing, many opinions; for opinion in good men is but knowledge in the making.

JOHN MILTON, *Areopagitica*

The relation between physiological factors and personal and intellectual factors is a subject about which much is surmised and little is known. To what extent does the mind depend on the body? This paper describes an experiment which attempts to throw some new light on this question which has been asked since time immemorial.

SOME INTELLECTUAL CORRELATES OF BIOCHEMICAL VARIABILITY[1]

by F. J. King, Barbara H. Bowman, and Harris J. Moreland

Florida State University and University of Texas

Some years ago Williams (1946) called attention to the fact that human beings vary widely in their metabolic patterns and suggested that the study of individual patterns might yield information of wider application than would the study of metabolism from a normative point of view only. Working in his laboratory, Berry, Cain, and Rogers (1951) found marked differences in the amino acid excretory patterns of adult males and females; in addition their data indicated that females excrete higher amounts of amino acids than do males. These results were replicated for both adults and children by Berry (1951) in both urinary and salivary amino acid patterns; and by Berry and Cain (1951) in a study of children. The latter authors raised the question of whether similarities and differences in amino acid excretory patterns are indicative of similarities and differences in mental and personality characteristics. Berry, Dobzhansky, Gartler, Levene, and Osborne (1955) and Gartler, Dobzhansky, and Berry (1955) studied the heritability of a number of amino acid excretion rates and presented evidence that indicated that several of them were apparently under genetic control. Gartler et al. (1955) suggested that metabolic variables such as these may have more importance in explaining certain adaptive qualities of their carriers than have the morphological characteristics which have been traditionally studied by geneticists and anthropologists. Cain (1951), investigating metabolic differences between normal and mentally deficient children, found four amino acids which differentiated between the normal and abnormal groups and five amino acids which differentiated between morons and imbeciles within the abnormal group. For all of these amino acids, imbeciles were found to excrete greater amounts than morons, who in turn excreted more than the normal subjects. Support was given these findings by Thelander and Imagawa (1956), who reported the presence of amino aciduria in mentally retarded children. In a more recent study, Stetten and Hearon (1959) found a low but significant correlation between intelligence and uric acid in a group of 817 service men.

The interpretation of differences between abnormal and normal groups in comparing excretion rates of various substances is difficult because of the possible interference of irrelevant variables. Thus consistent differences between the groups in habits of diet, elimination, amount of water intake, or perhaps even exercise might obscure real biochemical differences or might produce differences essentially unrelated to the condition being studied. For example, Zeller (1959) cited a study in which an observed chemical difference between schizophrenics and normals was due to the defecation habits of the schizophrenics. When these patients were treated with purgatives the differences disappeared while the pa-

[1] This study was supported in part by a grant from the Welch Foundation to Drs. C. P. Oliver and R. P. Wagner of the Department of Zoology, the University of Texas, and in part by a grant from the National Institutes of Health, No. RG-6492 (C1), awarded to Dr. W. S. Stone, also of the Department of Zoology at the University of Texas.

tients presumably remained in a state of illness. Control of as many as possible of these kinds of variables would, therefore, add considerably to the meaningfulness of the results.

Another possibility for refinement of previously accomplished studies lies in the choice of intellectual variables to be dealt with. Psychologists have long recognized that intelligence is not unitary in nature but rather a composite of a number of specific abilities. Consequently, studies on groups of subjects that are selected or measured by one global assessment of intelligence might fail to demonstrate important relationships between single aspects of intelligence and biochemical measures. If, for example, reasoning ability were related to a biochemical process but verbal ability were not, a complex test of intelligence that yielded one IQ score composed of reasoning, verbal ability, and other abilities might not show a significant relationship to the relevant biochemical variables.

A methodological improvement which could be made upon the studies cited would be the employment of an appropriate multivariate technique, such as canonical analysis, which could consider all the variables simultaneously. The calculation of numerous tests of significance would be avoided and the possibility of obtaining significant results by chance eliminated. In addition, a multivariate analysis might be capable of explicating functional patterns of relationships within and between the biochemical and psychological variates.

The purpose of the present research was to refine and extend the findings of Cain (1951), Thelander and Imagawa (1956), and Stetten and Hearon (1959). This was done through incorporating into the design of the study the improvements suggested above: use of only one relatively normal group of subjects, use of a number of psychological tests which have been designed to measure single ability traits (French, 1954), and use of a multivariate statistical approach.

TABLE 1

VARIABLES USED IN THE STUDY

Biochemical Variables[a]	Intellectual Variables[b]
1. Unidentified spot (2)	1. Speed of closure
2. Threonine methionine sulfone, proline (6)	a. Mutilated words
3. Asparagine, glutamine, citrulline (5)	b. Short words ?
4. Serine, homoserine (5)	c. Gestalt completion
5. Taurine, gamma-amino-butyric acid (2)	2. Verbal knowledge
6. Glutamic acid, aspartic acid (2)	a. Vocabulary
7. Glycine (2)	b. Verbal concepts
8. Alanine, alpha-amino-butyric acid (6)	3. Divergent thinking
9. Valine (6)	a. Seeing problems
10. Tryptophan (1)	b. Unusual uses
11. Phenylalanine (4)	c. Common situations
12. Glutamic acid (4)	4. Motor speed
13. Cystine, cysteine (3)	a. Writing lack
14. Ornithine (5)	b. Writing x's
15. Lysine (3)	c. Writing digits
16. Tyrosine, DOPA (3)	5. Associative memory
17. Norvaline (4)	a. First names
18. Proline (1)	b. Word number
19. Histidine, taurine, cysteine (3)	6. Number facility
20. Arginine, taurine, cysteine (5)	a. Addition
21. Cysteic acid (2)	b. Division
	7. Word fluency
	a. Rhymes
	b. Suffixes
	c. First and last letters

[a] The parenthetically enclosed number indicates the factor score of the amino acid.

[b] The three tests of divergent thinking and rhymes have been described by Guilford (1956), verbal concepts is a part of the California Test of Mental Maturity, and the remainder of the tests have been described by French (1954).

METHOD

Subjects were 58 children (36 boys and 22 girls) between the ages of 11 and 16 who were residents of a children's home in Texas. The average IQ of the group as measured by the California Test of Mental Maturity was 94, with a standard deviation of 14. No attempt was made to select subjects on any basis other than the requirement that they be free of any physical condition which might affect their normal amino acid excretory patterns.

TABLE 2

INTERCORRELATIONS OF TWENTY-ONE AMINO ACID VARIABLES

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21
1		10	16	18	39	35	−43	−04	−11	−11	−01	03	−10	−19	−11	−19	02	−16	00	−12	57
2			40	32	11	18	10	−22	26	−18	04	−05	01	02	04	−07	09	−32	−03	01	−06
3				52	14	21	10	−16	05	−18	17	−27	06	−07	18	09	06	−37	14	−28	−06
4					−10	09	40	−12	−03	−33	−02	−20	−11	−08	05	06	−02	−34	03	−47	−02
5						31	−38	−07	13	−09	33	18	00	−06	−08	−16	−02	−11	−14	19	35
6							−34	−23	−07	−22	01	−05	−12	−04	−06	−09	−03	−26	−02	18	46
7								−11	02	−18	−12	−11	−22	04	−08	−07	09	−17	−25	−39	−49
8									05	−13	−01	12	00	−04	−15	−12	11	−13	−09	06	−06
9										−03	03	14	06	14	01	−07	−06	−11	−04	11	−10
10											08	01	−13	−15	05	−06	−11	81	−19	−29	−24
11												16	09	13	−07	−11	47	−10	−12	06	23
12													29	13	−01	−20	25	−11	−12	12	29
13														27	61	56	10	−14	21	00	12
14															−20	−18	09	−12	04	14	03
15																68	−11	−07	27	−15	−20
16																	−17	−04	29	−09	−16
17																		−25	−26	06	28
18																			−21	−30	−24
19																				−03	−07
20																					20

Note—decimal points omitted.

Three early-morning urine samples were gathered from the subjects at intervals of approximately three weeks. On each day before a sample was taken, meals for all the subjects were prepared from the same menu. A battery of psychological tests was administered during the morning of each day a urine sample was taken. It was not possible, however, to examine every subject every time.

The technique employed for amino acid analysis for this project was the flurodinitrobenzene (FDNB) method (Levy, 1954). This technique is based upon the conversion of the free amino acids in the sample to their yellow N-2:4-dinitrophenyl (DNP) derivatives (Sanger, 1945, 1949). The conversion was accomplished by adding 0.1 ml FDNB to 4 ml of the sample having pH 9, adjusted with 1 M carbonate buffer. After the mixture was shaken for two hours in the dark, the excess FDNB was removed by ether extraction. The pH was adjusted to 2 with HCl, and the acidic amino acids were extracted with ether while the basic amino acids remained in the aqueous phase. After the acidic and basic amino acid extracts had been evaporated in the dark to dryness, they were suspended with acetone and water respectively. Following 2-dimensional separation on Whatman 1 filter paper using the solvents described by Levy (1954), the amino acids were eluted and the optical densities obtained by reading the eluate at 360 mμ (385 mμ in the case of

TABLE 3

ROTATED FACTOR MATRIX FOR 21 AMINO ACID VARIABLES

	I	II	III	IV	V	VI
1.	06	69*	−11	−02	−31	14
2.	21	03	01	03	−30	−54*
3.	17	12	16	−11	−52*	−45
4.	33	−08	02	−06	−74*	−06
5.	−05	54*	−13	14	02	−31
6.	21	60*	−14	−26	−08	−13
7.	23	−69*	−16	12	−49	04
8.	11	−14	−10	20	19	23*
9.	06	−13	−01	09	07	−37*
10.	−88*	−09	−05	07	01	−01
11.	−04	21	−03	47*	10	−34
12.	05	11	02	50*	21	03
13.	12	08	80*	29	20	−05
14.	18	−15	−02	10	25*	−23
15.	−09	01	82*	00	−14	−01
16.	−01	−04	76*	−18	−05	13
17.	21	05	−11	58*	02	−02
18.	−91*	−12	−09	−10	05	18
19.	14	09	40*	−38	11	−01
20.	26	17	−16	−08	61*	−10
21.	24	76*	−10	25	10	15

Note—decimal points omitted.

An asterisk indicates that the variable was used in the computation of the factor score.

dinitrophenyl proline). Table 1 shows the 21 DNP amino acids or groups of amino acid derivatives which were thus determined. Also given in Table 1 are the intellectual measures obtained during each of the three testing sessions.

For each subject the optical densities of each of the amino acid values were averaged. In order to control for sample dilution these averaged values were totaled and then each was divided by the total to obtain an amino acid/total amino acid ratio. For each subject the raw scores of each psychological test were converted to standard scores, and the scores of tests for each intellectual ability were averaged.

Because of the relatively small number of subjects it was desirable to reduce the number of variables to be considered in the analysis. Therefore, the 21 amino acid values for the 58 subjects were intercorrelated and factor analyzed by the centroid method and the quartimax method of rotation (Fruchter, 1954). This procedure also produced factor scores of greater reliabilities than the average reliabilities of their components (Guilford, 1950), and since each factor represented a relatively independent dimension of variability, minimized the correlations between the grouped variables. Rao and Slater (1948) have pointed out that this is a conservative procedure; the biochemical variables so grouped will explain somewhat less of the variance of the psychological measures than would be the case if each biochemical measure were used as a separate variable. Six factor scores were computed by the algebraic summation of the amino acid values contributing to each factor, no amino acid being allowed to contribute to more than one factor. The intercorrelations of amino acids are given in Table 2 and the rotated factors in Table 3. The six factor scores and the seven scores for intellectual abilities were intercorrelated for the 58 subjects for whom all the necessary data were available; the results are given in Table 4. A canonical analysis (Hotelling, 1936) of this correlation matrix was undertaken to determine the relationship between the set of intellectual variables and a set of five biochemical ones. The third factor variable was eliminated because it seemed to have little relation to the intellectual variables and also because the computations would be facilitated. The analysis was limited to the determination of the largest canonical correlation between the two sets of variates, which is defined as the square root of the first root of the equation, $|R_{ba}R_{aa}^{-1}R_{ab} - \lambda R_{bb}| = 0$, where R_{aa} is the matrix of intercorrelations of the biochemical variables, R_{bb} the intercorrelations of the intellectual variables, and R_{ab} the cross-correlations of the two sets. Also determined were the regression weights for each of the members of the two sets of variates. The approximate significance of the correlation may be determined by a formula developed by Bartlett (1948):

TABLE 4

INTERCORRELATIONS OF SEVEN INTELLECTUAL VARIABLES AND SIX AMINO ACID FACTORS

	1	2	3	4	5	6	7	I	II	III	IV	V	VI
1.		62	62	61	58	57	72	03	−31	−01	−15	16	−33
2.			53	48	44	60	66	07	−39	−02	−14	11	−06
3.				51	36	46	73	16	−42	04	−18	06	−25
4.					41	53	65	−11	−37	−09	−11	16	−16
5.						25	57	−07	−35	−11	−24	16	−13
6.							55	−08	−24	09	00	11	−07
7.								01	−33	−05	−20	28	−20
I.									−03	−28	−19	−18	08
II.										10	28	14	02
III.											−10	−16	11
IV.												04	−07
V.													−09

Note—decimal points omitted.

$\chi^2 = -(n - \frac{1}{2}(p + q + 1)) \log_e(1 - \lambda_1)$ with $(q - p + 9)$ degrees of freedom where n equals $N - 1$, p equals the number of the smallest set of variates, and q the number of the largest set.

RESULTS AND DISCUSSION

The canonical correlation between the biochemical and the intellectual variables was found to be .599 which was significant at beyond the .025 level. The regression weights for the five biochemical variables were 1.00, −5.49, −1.23, 2.35, −2.36 respectively, and for the seven intellectual variables .00, 1.13, 2.37, 1.14, 1.39, −.90, −.31. The correlation may be interpreted as indicating the amount of common variance shared by one dimension or component of the psychological variables with one dimension or component of the biochemical variables. The canonical weights would probably be roughly proportional to the saturations of the respective variables on one component of a principal components matrix when each set of variables was analyzed separately (Burt, 1948).

Because the natures of regressions of psychological variables on biochemical ones have not been previously studied, checks were made on the linearity of these regressions through the use of the test outlined by Dixon and Massey (1957). There were good indications that some of the relationships studied were curvilinear, but because the small number of subjects precluded the determination of the forms of the curves, no attempts were made to find linear transformations for the relationships. Consequently, the cross-correlations for the two sets of variables probably represent conservative estimations of the common variance of the two sets.

While the results of this study cannot be compared directly with the studies of Cain (1951) and Thelander and Imagawa (1956), the negative direction of three of the five biochemical weights seems to support their general findings of negative relationships between intellectual level and excretion rates for several amino acids. Although the present findings do not necessarily demonstrate the existence of causal relationships between the two sets of variates, studies based upon this one might well do so and might also produce findings of considerable theoretical and practical importance.

For example, inconsistencies found in the results of studies where either glutamic acid (Waelsch, 1951; Weil-Malherbe, 1950) or glutamine (Rogers & Pelton, 1957) was fed in an attempt to raise intellectual functioning in mentally retarded children might well be due to the fact that therapy was administered without regard to individual amino acid excretion patterns and individual patterns of intellectual abilities in the subjects. Studies using unique amino acid feedings based on individual configurations of biochemical and mental characteristics could be devised to explore this area with a high degree of certainty, with consistent gains in specific abilities pointing to causal relationships between the two sets of variates.

Through work with subjects of one sex at specific age levels, it would be possible to determine whether some of the variance common to biochemical and psychological variates in the present study was due to sex or age differences in excretion levels and psychometric performance. It is possible, too, that certain intellectual abilities might be found to be closely related to distinct biochemical patterns while others might be relatively independent of them. In connection with studies of heritability of patterns of amino acid metabolism, studies of this sort could lead to greater understanding of the roles of heredity and environment in the development of intellectual abilities.

REFERENCES

Bartlett, M. S. Internal and external factor analysis. *Brit. J. Psy. Stat. Sec.*, 1948, 1, 73–81.

Berry, Helen. Further studies on individual urinary and salivary amino acid patterns. In *Biochemical institute studies IV*. Univ. of Texas Publication No. 5109, 1951, 157–164.

Berry, Helen K., & Cain, Louise. Individual excretion patterns of young children. In *Biochemical institute studies IV*. Univ. of Texas Publication No. 5109, 1951, 165–172.

Berry, Helen K., Cain, Louise, & Rogers, Lorene L. A study of the urinary excretion patterns of six human individuals. In *Biochemical*

institute studies IV. Univ. of Texas Publication No. 5109, 1951, 151–156.

Berry, Helen K., Dobzhansky, T., Gartler, S. M., Levene, H., & Osborne, R. H. Chromatographic studies on urinary excretion patterns in monozygotic and dizygotic twins. I. Methods and analysis. *Amer. J. hum. Gen.*, 1955, 7, 93–107.

Burt, C. Factor analysis and canonical correlations. *Brit. J. Psy. Stat. Sec.*, 1948, 1, 95–160.

Cain, Louise. Exploration of metabolic patterns in mentally deficient children. In *Biochemical institute studies IV*. Univ. of Texas Publication No. 5109, 1951, 193–205.

Dixon, W. J., & Massey, F. J. *Introduction to statistical analysis*. New York: McGraw-Hill, 1957.

French, J. W. (Ed.) *Manual for kit of selected tests for reference aptitude and achievement factors*. Princeton, N. J.: Educational Testing Service, 1954.

Fruchter, B. F. *Introduction to factor analysis*. D. Van Nostrand Co., Inc., 1954.

Gartler, S. L., Dobzhansky, T., & Berry, Helen K. Chromatographic studies on urinary excretion patterns in monozygotic and dizygotic twins. II. Heritability of the excretion rates of certain substances. *Amer. J. hum. Gen.*, 1955, 7, 108–121.

Guilford, J. P. *Fundamental statistics in psychology and education*. New York: McGraw-Hill, 1950.

Guilford, J. P. Relation of intellectual factors to creative thinking in science. In C. W. Taylor (Ed.), *1955 University of Utah research conference on the identification of creative scientific talent*. Univ. of Utah Press, 1956, 69–95.

Hotelling, H. Relations between two sets of variables. *Biometrika*, 1936, 28, 321.

Levy, A. L. A paper chromatographic method for the quantitative estimation of amino acids. *Nature*, 1954, 174, 126–127.

Rao, C. R., & Slater, P. Multivariate analysis applied to differences between neurotic groups. *Brit. J. Psy. Stat. Sec.*, 1948, 1, 17–29.

Rogers, Lorene L., & Pelton, R. B. Effect of glutamine on IQ scores of mentally deficient children. *Texas Repts. Biol. Med.*, 1957, 15, 84–90.

Sanger, F. The free amino acid groups of insulin. *Biochem. J.*, 1945, 39, 507–515.

Sanger, F. The terminal peptides of insulin. *Biochem. J.*, 1949, 45, 563–574.

Stetten, D., & Hearon, J. T. Intellectual level measured by army classification battery and serum uric acid concentration. *Science*, 1959, 129, 17–37.

Thelander, H. E., & Imagawa, R. Amino aciduria, congenital defects, and mental retardation. *J. Ped.*, 1956, 49, 123–238.

Waelsch, H. Glutamic acid and cerebral function. *Advances protein Chem.*, 1951, 6, 299–341.

Weil-Malherbe, H. Significance of glutamic acid for the metabolism of nervous tissue. *Physical Rev.*, 1950, 30, 549–568.

Williams, R. J. *The human frontier*. New York: Harcourt, Brace, 1946.

Zeller, E. A. Tryptophan metabolism in schizophrenics and controls. In R. A. Gibbs (Ed.), *Molecules and mental health*. Chicago: J. B. Lippincott, 1959, 60–64.

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Obstinancy and heat in sticking to one's opinions is the surest proof of stupidity. Is there anything so cocksure, so immovable, so disdainful, so contemplative, so solemn and serious as an ass?

MICHEL EYQUEM DE MONTAIGNE, *Of the Art of Conversing*

Each year, doctors, engineers, economists, scientists, educators, and technicians of all kinds go from the United States into underdeveloped areas in South America, Africa, and Asia, to act as consultants in planning programs of development in these countries. The success or failure of these plans does not depend entirely on the technical abilities and skills of the consultant, but is affected—sometimes adversely—by the emotional factors present in patterns of interaction between consultants and consultees. This paper discusses the problems of such emotional interaction in Israel, and describes methods for solving them.

PATTERNS OF INTERACTION BETWEEN CONSULTANTS AND CONSULTEES IN ISRAEL

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In a comprehensive study, Caplan (1960) has reported on his experience and theoretical conclusions with regard to the patterns of interaction between technical experts who operate in foreign countries as consultants, and consultee institutions and individuals. His assumption that there is in all probability a regular pattern of interaction, shaped by the inevitable differences in the specific conditions in each country, deserves further research and elaboration.

Experience indicates that the ideas and plans of experts operating in foreign countries are seldom accepted or rejected on the sole basis of their objective value. The success or failure of the expert's plan of action is not only based upon his specialized skills and knowledge, but is very much dependent on attitudes emanating from psychosocial factors in the background of consultants and consultees.

The case of experts working in Israel has been discussed thoroughly by Caplan in the above-mentioned paper. Israel offers an excellent opportunity to study this problem, both because of the increasing number of specialists required to fulfill the expanding needs of the new state and because of the possibility of a common technical language for the foreign expert and the individual(s) seeking advice. No visible reason appears, then, to explain the eventual failure to implement a plan advised by a skilled expert to meet some urgent need of the country. Israel provides, therefore, an excellent opportunity to illustrate the problem of emotional factors present in the interaction between foreign consultants and consultees. Yet, we have to be aware of the fact that no general conclusions may be drawn from experience limited to only one case, especially if this particular instance deals with such a peculiar country as Israel.

Among several specific characteristics of Israel, three factors deserve special emphasis because of their vast influence upon any plan of action designed by the consultant. One of these factors is the continuing change in every aspect of life as a consequence of the free immigration of Jews arriving from the most varied countries and cultures all over the world. This presents the difficult problem of acculturation and integration of a large mass of immigrants who bring with them a widely differing baggage of language, customs, and values, in addition to their different motives for coming to Israel. The process of acculturation of the immigrants is particularly difficult because of the relative weakness of the already settled nucleus which must absorb the newcomers. It may be worthwhile to recall that the ratio between newcomers and "old" immigrants who settled in Israel

twelve or more years ago is about 3:1 in favor of the new immigrants.

A second factor to be emphasized is the scarcity of resources and the considerable financial restrictions, which bring about a striking discrepancy between means and tasks. This accounts at least partly for the large number of attempts at short-range solutions; for the use of provisional and inadequate measures to face emergency situations instead of systematic but expensive long-range planning.

Finally, the complexities of the process of acculturation under constant pressure of time and scarcity of resources are magnified by an additional factor, namely, the element of insecurity, which comes from the continued state of tension along Israel's borders and the threatening menace of a war.

All these specific elements must necessarily be considered by the consultant in any plan of action, which should not be a kind of mechanistic transfer of systems that proved to be adequate elsewhere. One may say that the most important factor of success—providing that the consultant has the required skills and knowledge—is the correct understanding of both the specific needs to be met and the concrete possibilities that the plan can be carried out by those who are to implement the new program. Experience has shown, however, that diverse factors may distort the usual ability of judgment not only of the visiting consultant but of the consultee too, interfering with the required realistic appraisal of the present conditions. This has been a very common observation, which was repeatedly expressed, for instance, by the participants of the workshop devoted to the Kibbutz system of education at the 1957 Annual Meeting of the American Orthopsychiatric Association (1958).

Hitherto, the scattered pieces of analysis of this subject have been provided by the consultants themselves, after a rather short stay and experience in the country. To the best of my knowledge, the problem has been only incidentally touched on the other level, that is, from the point of view of the consultee. The author, who has lived in Israel since before the establishment of the young state, has considered the importance of an integrated attempt to explore the problem, including the views and reactions of both sides. This exploration was facilitated by three factors: (1) The personal experience and contact of the writer in Israel with several experts from English-speaking countries; (2) As a member of the Health Advisory Council attached to the Health Ministry in Israel, the author had a chance to discuss this problem and its implications with the consultees themselves; (3) The author's recent stay in the United States provided him the opportunity to contact several experts who had visited Israel as consultants.

The author met with the experts from English-speaking countries individually in rather unstructured interviews to ascertain the possible reasons for success or failure, with particular stress on the emotional factors involved in the implementation of the consultants' tasks. The group which met in the United States included six experts who had served as consultants in Israel at different times and in different fields (mental health, medicine, public administration, economics, and industry). A second series of meetings was held with a number of Israeli citizens who were visiting the United States, and who acted in their country as consultees in relation to consultants from English-speaking countries. Likewise, these Israeli consultees were encouraged to indicate the nature of the psychosocial factors that contributed to the success or lack of success of the consultant. In a parallel way, the administrator of one of the offices in charge of contracting experts from English-speaking countries, with several years of experience both in Israel and in the United States, was interviewed on the same subject.[1] A fairly positive agreement was found in the separate evaluations at the consultant, consultee, and administrative levels, enabling the author to integrate rather smoothly the data presented by different sources of information.

All the consultants interviewed (by the

[1] I am indebted to Mr. Isaac Woolfson, Administrator of Patwa Organization, for his valuable information and advice.

way, all of them were Jews) found to a greater or lesser extent that diverse emotional factors accompanied their objective tasks. The degree of awareness and insight varied in each case. Sometimes the recognition of those factors came only after the expert's mission was already accomplished.

EMOTIONAL FACTORS INVOLVED

One of the most common situations seems to be the exaggerated emotional involvement with Israel's tasks and ideals, leading to an overcommitment and contagion of emotions and attitudes. A characteristic example is that described by several scientific researchers and consultants of English-speaking countries, who went to Israel to study the communal settlements (Kibbutz Movement). After a short stay there they felt compelled (without any external pressure or suggestion) to combine their scientific task with some kind of physical work along the needs of the Kibbutz (Spiro, 1956; Rosenfeld, 1958). As Rosenfeld (1958) puts it, "There is an almost uninterrupted state of military and social emergency in the state of Israel . . . The situational pressures for emotional involvement with the community in crisis become so great that we feel guilty if we do not become involved. There comes a moment when we feel ashamed to take notes, ashamed to observe. . . ."

Emotional involvement of the consultant

The emotional involvement may lead to an unwitting distortion of reality on the part of the consultant. An idealistic overvaluation of Israel's assets and possibilities together with a feeling of Messianic mission combine to develop plans of action which are not appropriate for the specific conditions. This seems to be one of the explanations for the apparent contradiction between the eagerness with which the consultant's advice is sought and the frequent lack of implementation of his plan.

Caplan (1960) assumes that in Israel there is a greater resistance than in other countries to changing ways of operation and suggests as a possible explanation that "a much higher proportion of people in Israel than in many other countries have invested their occupational or professional activities with great emotional intensity." This is only an impression and it is rather difficult to attempt to prove or to contradict this statement. The author's own impression is just the opposite. The fact that the country is in constant process of change and development rather discourages personal stagnation within fixed systems of action. A well-established fact in social psychology is that critical situations, as represented by conditions of prolonged insecurity or some other kind of crisis, make men "unusually susceptible to the acceptance of new formulations, whether or not these new formulations afford objective and lasting solutions . . . The acceptance of new ideas and values is substantially enhanced by the instability, fluidity, and vagueness of the situations that the individuals face" (Sherif, 1948).

It is hard to believe that the implementation of program planning advised by Israeli or foreign consultants faces more obstacles in Israel than in other places. The experience of the last twelve years seems to prove the opposite. New ideas, plans, and attitudes that affect the individual as well as the social group find fertile ground. Only a few years ago the practice of polygamy carried on for centuries among the Yemenite Jews and Arab minority was seen as normal for these cultures. The use of physicians in case of illness was almost unknown. Women delivered their children lying down on a muddy floor, and refused to be attended by a physician. However, radical changes have been accomplished with much less resistance than would be expected in another country enjoying normal conditions and some degree of stability. After a relatively short period of planning, new programs have been implemented in every aspect of Israeli life, such as education, health, defense, industry, agriculture, etc., in spite of the anticipated resistance.

The emotional overcommitment on the part of the consultant may lead him to disregard realistic considerations and obstacles and to consider them—at the time of their

appearance—as expressions of resistance to changes. Frequently, the emotional involvement is expressed in the form of idealization of the object. A hidden dissatisfaction with the personal ego-ideal fulfillment is dealt with in an unrealistic and illusory overvaluation of Israel's potential. Through the mechanism of idealization, the consultant may set exaggerated standards of perfection which eventually will be reflected in his way of planning. When the idealized image comes tumbling down because of discrepancies that can no longer be ignored, disappointment and serious disillusionment may result.

The interference of idealization of Israel with the objective plans of the foreign expert was easily admitted in retrospect by all the consultants. We assume that no mere coincidence brought about the differences between the idealized version of the creation of the state of Israel as related in an American best-seller book (Uris, 1958) and the more realistic Israeli literary descriptions of the same subject.

As stated by Caplan (1960), Rosenfeld (1958), and others, the Zionist consultant may carry a hidden sense of guilt about not immigrating to Israel. The assumption that one of the reasons to reject the consultant's ideas may be "the suspicion that he may wish to stay on in the country . . . and displace the person he is advising" (Caplan, 1960) seems to be pure projection of the consultant's own conflict, a projective defense to cover that sense of guilt. Another defensive device to offset the feelings of anxiety and guilt in the areas of identification with and separation from the ideals of Israel's rebirth is the use of isolation and detachment to separate the consultant's plans and actions from any emotional context. He will try to function as a pure consultant, as a scientist concerned only with reality, figures, and facts. This kind of realistic approach with emotional undercommitment may result in plans that fail to meet the current needs. The realistic limits seen by the consultant and the "reality-bound" plan of action may not necessarily match the expanded concept of reality in a pioneering society. A popular Israeli aphorism stresses the fact that if Israel's development had followed the realistic advice of experts, the state could never have been created. The majority of the experts interviewed by the author were able to recognize that one of the most difficult tasks that the consultant must face in Israel is to find the middle road between Messianic, omnipotent fantasies and rigid attachment to static reality.

Emotional involvement of the consultees

Through our inquiry both consultants and consultees were able to discern the presence of open or disguised feelings of resentment and resistance to the consultant on the part of the consultees. This seems to be a common feature unrelated to the consultant's attitudes and stemming from the possible presence of in-group feelings shared by the members of the Israeli group with unwitting hostile feelings toward outsiders.

There are several illustrations in the field of social psychology to corroborate the common experience that deprivation or frustration encourages the formation of a significant group-pattern of feeling from which outsiders are not only excluded but may eventually become the target of hostility. The experimental study of prolonged semistarvation carried out at the University of Minnesota, the psychological implications of which were presented by Guetzkow and Bowman (1946), seems to furnish clear-cut evidence in this respect. The subjects, all of them volunteers, who were systematically subjected to semistarvation for six months, built up a striking group-feeling that tended to exclude their nonstarving friends. Even the men who managed to continue their social contacts often felt animosity towards strangers, merely because they were strangers.

The formation of an in-group feeling within different sectors of the Israeli population is very clear to the unbiased observer. This fact, for instance, has been frequently reported by researchers and simple visitors to the Kibbutz Movement who find it very difficult to break down the barriers that separate the *them* from the *us*. There is a powerful group-feeling; expressed,

among other ways, by the conviction that outsiders cannot understand the intricacies of the group experience, since they do not share the Kibbutz conditions of hard sacrifices and deprivation. To a lesser or larger extent the same is valid for other Israeli groups which face the hardships and the difficult tasks of building a new state in conditions of poverty, deprivation, and insecurity. The inner solidarity of the group may be interpreted by the outsider as rejection. On the other hand, the "congenial" group may actually develop feelings of hostility toward the stranger, especially a "close stranger" (a Zionist consultant in our case), who is eventually seen by the group—on an unconscious or conscious level—as a deserter failing to share the common lot of self-sacrifice. This hostility may eventually increase if the consultant's unawareness of the roots of these "ungrateful" in-group feelings makes him react by verbally stressing his generous support and co-operation.

CONCLUSION

When factors like emotional overcommitment, Messianic omnipotence, overidealization, guilt-feeling leading either to projective devices or isolation, and hostile in-group feelings, were recognized and worked through in the interaction between consultants and consultees, the full use of the skills and abilities of the consultant could be assured.

REFERENCES

Caplan, G. Some problems of technical consultation in Israel. Read at 37th Annual Meeting, Am. Orthopsychiat. Ass., Chicago, February 1960.

Guetzkow, H. S., & Bowman, P. H. Men and hunger: a psychological manual for relief workers. Elgin, Ill: Brethren Publishers, 1946.

Rosenfeld, Eva. The American social scientist in Israel: a case study in role conflict. *Am. J. Orthopsychiat.*, July 1958, 28, 563–571.

Sherif, M. *An outline of social psychology*. New York: Harper & Brothers, 1948.

Spiro, M. E. *Kibbutz: venture in utopia*. Cambridge, Mass.: Harvard Univ. Press, 1956.

Uris, L. *Exodus*. New York: Doubleday, 1958.

Karpe, R. (Ed.), Behavior research in collective settlements in Israel. *Am. J. Orthopsychiat.*, July 1958, 28, 547–597.

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Objections, nonsequiters, cheerful distrust, joyous mockery—all are signs of health. Everything else belongs in the realm of pathology.

FREDERICK NEITZCHE, *Aphorisms*

The effective distance between two places or objects, or individuals, cannot be measured in miles. It depends not only on physical distance but on economic distance (measured in terms of transport cost), communication distance (cost, efficiency, frequency of use and exchange of telephone calls, newspapers, radio and television programs, letters, etc.), social distance (amount and type of interaction between individuals or groups), and others. Also, the distance may be different for each kind of transaction. Described here is a method for computing effective distance, and some suggestions for further investigation of the concept.

A NOTE ON A GENERALIZED CONCEPT OF EFFECTIVE DISTANCE

by Karl W. Deutsch and Walter Isard

Yale University and University of Pennsylvania

DISTANCE is usually conceived of as geographic distance in terms of miles and is used accordingly for mapping, for flight computations, and related purposes. Economists use a more sophisticated concept of "economic distance," which is measured in terms of transport cost (Isard, 1956), for analysis of location and space. The economic distance between two points is then the cost of transporting a commodity from one to the other. Since effective transport rates differ from commodity to commodity, the resulting computations of economic distance differ for each commodity. Writers such as Chauncy Harris (1954) have tried, however, to construct an average transport cost for an aggregate class of commodity flows.

Similarly, more-or-less sophisticated notions of distance apply to communications. The telephone distance between two places is not always the distance as the crow flies, but may involve the shortest distance in the network of available telephone lines. It may have to be weighted not only by a cost factor, such as the rate set by the telephone company, but also for expected delays and frequency overloads.

Sociologists have used a concept of "social distance" emphasizing the interaction among members of similar or different social groups or cultures. "For example, the analyst thinking in terms of social distance recognizes that the Puerto Rican who migrates to New York is, from a social distance standpoint, migrating to the closest location of significance to him." (Isard, 1960, p. 542). Some sociologists have used the concept of intervening opportunities to amalgamate economic and social distance. Other sociologists have attempted to measure the possibility of interactions and of friendly relations between ethnic and racial groups by constructing a "social distance scale," based upon interview materials and responses to questionnaires. (Bogardus, 1933; Isard, 1960, p. 506, 542–544; Klineberg, 1950, p. 135).

A number of social scientists have tried to trace the effects of distance on the frequency of a given kind of transaction (interchange, interrelations, etc.) between actors. The most frequent relation that has been asserted resembles the well-known mutual energy concept of gravitational (Newtonian) physics. The frequency of transactions, it has often been suggested, ought to be directly proportional to the product of the masses of the two actors and inversely proportional to the distance, d, separating these actors. The simplest formula for predicting transactions between two spatially separated actors or categories is thus

$$G\frac{M_i M_j}{d_{ij}}.$$

Here G is a constant, and M_i and M_j repre-

sent the two masses, d_{ij} being the intervening distance. M_i and M_j may refer to the population of two cities, or to the incomes of two cities (or of two groups of actors, or simply two individuals), or to telephone installations of two metropolitan areas, or other relevant political, economic, and social magnitudes.

Slightly more complicated models would raise d_{ij} to a power other than unity, such as two, or even consider the exponent of d_{ij} as a variable. Other models might (1) weight the masses by a simple measure (such as the average number of years of schooling), or by an index constructed from several properly weighted measures; and (2) raise each mass itself to a constant or even variable power.[1] Finally, they may even employ a different mathematical formulation than the above to encompass the attenuating influence of distance. It seems clear, however, that the different ways of defining and computing distance will have an effect on the attempts to predict the volume of a given kind of transaction for a given pair of actors.

One of the purposes of the present paper is to suggest that it might be worthwhile to invert the implied computational procedure, and by so doing overcome the difficulty of accounting separately for the many different economic, social, and political factors bearing upon the concept of distance. For example, suppose we view the flow of economic transactions, T_{ij}, as the prime indicator of interaction, and initially employ the simplest model, namely, $T_{ij} = G\dfrac{M_iM_j}{d_{ij}}$. Then, if relevant characteristics of populations (actors) and the volume of given transaction flows are available, it ought to be possible to use these known data to make a first approximation of the effective distance between the two actors or actor categories. The old procedure takes the estimate of distance as given and treats the transaction flow as the unknown to be found. The method proposed here treats past data about transaction flows and known data for the relevant populations as given, in order to determine what the effective value of distance between the actors may have been for the period in question.[2] It is explicitly taken for granted that the two variables, transaction flows and population characteristics of each of the actors, can be established independently by standard methods.

The resulting value for effective distance between any two actor categories, however imperfect this value may be, does obviate the difficulty of obtaining standardized methods, theoretically satisfying to the different social science disciplines and investigators, which will reflect approximately the relative significance of transport costs, cultural affinity, political bonds, legal channels, credit availability, and the host of other factors relevant for the definition of distance. In essence, our procedure may be viewed as yielding an estimate of "effective distance" as some weighted average of many component elements where it is fully recognized that different weightings of the elements can give the same average result; and it is only this average result which the proposed method of computation would measure. The determination of the basic component elements and their proper weights still would wait upon major theoretical advances in the several social sciences.

Mathematically put, we define the effective distance d_{ij} between actor categories (places) i and j as:

$$d_{ij} = \mathbf{x}_{ij} \cdot \mathbf{w}_{ij}$$

Here $\mathbf{x}_{ij}$ is a vector in n dimensional space.

[1] The most rigorous form of the relation indicating interaction, I_{ij}, between i and j is:

$$I_{ij} = G\frac{w_i(P_i)^{\alpha} \cdot w_j(P_j)^{\beta}}{d_{ij}^{b}}$$

where w_i and w_j are the weights applied to the respective masses, where α and β are exponents applied to the respective masses, and where b is the exponent (constant or variable) applied to distance. For a discussion of such formulas, see Isard (1960, p. 493–566). The formula cited is on page 510; a survey of the literature is given on pages 566–568.

[2] On the general problem of distance for estimating the political, economic, and social relationships between actors, see Deutsch (1954, p. 59–60). For the general potentialities of distance for the study of international politics, see also Deutsch (1960).

Each component of this vector measures one aspect of distance; e.g., the first one may be said to measure physical distance; the second one, to measure economic (transport cost) distance; the third one, to measure political distance as indicated by quotas and other trade restrictions; the fourth one, social distance as measured by attendance at certain meetings, etc. Also, $\mathbf{w}_{ij}$ is a weight vector in n dimensional space, each of its components being a weight to be applied to the corresponding elements of $\mathbf{x}_{ij}$. Thus, the first element of $\mathbf{x}_{ij}$ is to be multiplied by the first element of $\mathbf{w}_{ij}$, the second by the second, and finally the nth by the nth. The value d_{ij} is obtained as the sum of the resulting n products. From this computation it is clear that in general many $\mathbf{w}_{ij}$ vectors can yield that value of d_{ij} which it is our purpose to estimate roughly from empirical study.

It is to be reiterated that our approach does recognize that the distance between two cities or two countries or two ethnic groups may, of course, be different for each different kind of transaction. It may turn out that the distance between Egypt and Syria in terms of international trade in 1951 was different from the distance in terms of the exchange of letters between those two countries, and still another distance figure might result from a computation based on the frequency of travel or migration.[3]

Inquiries of this kind might lead to a more critical view of the entire distance concept. The naive notion of distance as expressed solely in miles assumed that all these different operational definitions of distances would, for practical purposes, coincide. The same assumption was implied in the even older but slightly less naive idea of measuring distance in terms of hours or days of travel, which implied a primitive sort of weight for different kinds of terrain. All these traditional measures suggested that the distances relevant for trade, for travel, for intermarriage, for communication, or for military affairs all were somehow related to each other. If two towns were neighbors in terms of one of these transactions, it was thought that they were very likely to behave as neighbors in all of them.

Whether modern countries do or do not behave in a similar way is precisely one of the things that could be found out by applying the method proposed in this paper. Our suspicion is that they do not, and this question can be examined more meaningfully once we have measures of effective distance based upon transaction data. We may find out, in other words, whether Englishmen and Irishmen, or Americans and Puerto Ricans, or Americans and Cubans, are as close to each other in terms of trade as they are in terms of travel, or in terms of communication, or in terms of intermarriage, or in terms of their radio broadcasts and newspapers.

As so often in social science, some of the most interesting results might appear in the deviant cases, that is, cases of countries or groups that are very close in some kinds of transactions but very distant in others. Colonies, for example, might be much closer to the metropolitan country in economic than in geographic distance, but much farther in social distance as it applies to a significant range of human relations. An analysis of such deviant cases might tell us something about possible situations worthy of particular attention.[4]

[3] This is a consideration quite different from the consideration of different weights attached to distances in the earlier case discussed above in the text, where weights are attached to transport costs, availability of credit, etc., all in terms of their effects on the same transaction category of, say, trade. In the problem discussed now, we are dealing with the different distances, the weighting of which may or may not have changed, for each different category of transactions.

[4] On the general utility of deviant case analysis, see Lazarsfeld & Rosenberg (1955).

REFERENCES

Bogardus, E. S. A social distance scale. *Sociol. and soc. Res.*, 1933, 17, p. 265–271.

Deutsch, K. W. *Political community at the international level*. New York: Doubleday, 1954.

Deutsch, K. W. Toward an inventory of basic trends and patterns in comparative and international politics. *Amer. pol. sci. Rev.*, 1960, 54, 34–57.

Harris, C. D. The market as a factor in the localization of industry in the United States. *Annals Assoc. Amer. Geographers*, 1954, 44.

Isard, W. *Location and space-economy*. New York: John Wiley, 1956.

Isard, W. *Methods of regional analysis: an introduction to regional science*. New York: John Wiley, 1960.

Klineberg, O. *Tensions affecting international understanding: a survey of research*. New York: Social Science Research Council, 1950.

Lazersfeld, P. F., & Rosenberg, M. (Eds.), *The language of social research*. Section II-C. *Deviant case analysis*. Glencoe: Free Press, 1955.

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It is said that Mirabeau took to highway robbery "to ascertain what degree of resolution was necessary in order to place one's self in formal opposition to the most sacred laws of society." . . . This was manly, as the world goes; and yet it was idle, if not desperate. A saner man would have found himself often enough "in formal opposition" to what are deemed "the most sacred laws of society," through obedience to yet more sacred laws, and so have tested his resolution without going out of his way. It is not for a man to put himself in such an attitude to society, but to maintain himself in whatever attitude he find himself through obedience to the laws of his being, which will never be one of opposition to a just government, if he should chance to meet with such.

HENRY DAVID THOREAU, *Walden*

Dr. Wenger, one of the authors, explains this article as follows:

"In 1933, a student from India first stimulated my interest in Yoga. In an experiment on muscular relaxation his performance far exceeded that of any other subject. He explained that he employed a Yogic method which was commonly used in India, and I made a mental note to go to India someday. The note got unburied over twenty years later when I discovered an article by Thérèse Brosse, a French cardiologist who had taken a portable electrocardiograph to India in 1935 and measured a few yogis as they attempted to control their heart action. One of her published EKG records was amazing. It showed a gradual reduction in heart potentials to near zero. She concluded that the heart could be controlled voluntarily.

A review of the literature disclosed similar claims, and other claims of voluntary control over other visceral muscles—none, however, so objectively documented. Claims of fire walking, pit burials of many hours' duration, and needle penetration without bleeding are fairly common. Less common are reports of voluntary control of regurgitation and defecation, of great increases in body heat, and of relaxation of the sphincters of the anus and urethra so that water or other fluids can be sucked into the bowel or bladder with the aid of another practice (*uddiyana*) that apparently creates negative pressure in the abdomen.

Testing these various claims of voluntary control of autonomic functions and recording physiological changes during Yogic meditation were our major purposes in going to India."

STUDIES OF AUTONOMIC FUNCTIONS IN PRACTITIONERS OF YOGA IN INDIA[1]

by M. A. Wenger and B. K. Bagchi

University of California at Los Angeles and The University of Michigan

For five months early in 1957 the authors traveled in India on a somewhat novel research venture. The major area of interest was Yoga; the general purpose was to conduct psychophysiological studies on yogis.

We took with us an 8-channel portable Offner transistor electroencephalograph with appropriate pickups for d.c. recording of ANS functions.[2] We established five different laboratories (Calcutta, Madras, Lonavla, and two in New Delhi); and made additional recordings in two private homes and one mountain cave (Bagchi & Wenger, 1957, 1959). We obtained recorded data from 43 male and 2 female subjects, all yogis or students of Yoga; and in many instances repeated experiments were possible with the same subject.

A brief note on Yoga seems in order. It is an important aspect of Indian culture, and was first mentioned in the early Upanishad literature (*circa* 700 B.C.). There have been many philosophical concepts, practices, and branches of Yoga; an underlying similarity exists, however, in that all emphasize self-culture, self-realization. Yoga, then, is a form of religion. After the Upanishad period when Yoga was systematized as a

[1] This work was made possible by grants from the Rockefeller Foundation, the Rackham Foundation of The University of Michigan, and subcontract AF18(600)-1180 from George Washington University; and was sponsored by the Indian Council of Medical Research. Individual acknowledgments have been recorded elsewhere. (Wenger, Bagchi & Anand, in press; Bagchi & Wenger, 1957, 1959; Wenger & Bagchi, mimeographed ms.)

[2] Cf. Wenger (1948), Wenger, Engel, & Clemens (1957) for descriptions of the accessory apparatus.

major branch of Indian philosophy, Patanjali's book *Yoga Sutra* (*circa* 500 B.C.), supposed to be the most authoritative early treatise on Yoga, accepted the theoretical concepts of Kapil's Sankhya philosophy, which is not pantheism but dualistic realism. To this, Patanjali's Yoga philosophy added the concept of modified theism. Patanjali's special emphasis was on elaboration of the requisites and steps of Yogic practice.

The word Yoga has different meanings. It is related to our word "yoke". One meaning of Yoga is union (being yoked) with God. Patanjali does not speak of union with God; to him Yoga was stopping the disturbances of the *Chitta* (mind) so that the individual soul could be in its own pristine state of freedom. Another meaning of Yoga is equilibrium; still another, skill in action. The word is used to signify either the end or the means. The end, according to Patanjali, is *nirvikalpa samadhi* (unconditioned meditation, "state of illumination", "freedom", "self-realization"). Yoga is held to bring with it certain powers uncommon among men

Since sensations arising from the body interfere with deep meditation, health and internal cleanliness become of prime importance for the yogi. To further this end one branch of Yoga, known as *Hatha Yoga*, has evolved systematic postural and respiratory exercises (*asana* and *pranayama*). Most of the claims for voluntary control over visceral functions are made by those who practice Hatha Yoga.

It should be pointed out that many Indians and others engage in these exercises for physical culture only, and some of our subjects fall in this class. A few use them for exhibition purposes, and two of our subjects might be so classified. Most were sincere followers of the path they had chosen; and most gave us excellent co-operation. Some yogis, however, refused any form of co-operation, even conversation.

RESULTS AND DISCUSSION

Since our work was exploratory and necessarily fragmentary in nature the results are reported in separate sections and discussed immediately.

Voluntary control over visceral functions

Only one subject was found who possessed direct voluntary control over any autonomically innervated function. He could perspire from the forehead on command within 1½ to 10 minutes. The act was accompanied by marked increase in systolic blood pressure of which he was unaware; by slight increases, then decreases in forehead skin temperature (presumably evaporation effects); by marked drops in forehead but only slight drops in palmar skin resistance; and by slight increases in arm skin temperature. No detectable movements occurred.

The data we recorded from this subject are presented in Table 1. All represent intermittent measurements. A strap was devised to hold the forehead electrodes firmly in place above the two eyes. Skin temperatures and room temperature were recorded by a multiple thermistor unit.[3] On first measurement the subject, who previously had served in other experiments, wanted as little apparatus attached as possible. On retest at the first possible opportunity, he permitted a blood pressure cuff and one additional thermistor, but wanted one hand free and no other apparatus. A third test with more variables proved impossible; thus, although auscultation indicated increased heart rate on the second test, no objective records are available.

This man had spent parts of two winters in caves in the Himalayas. During such periods, usually alone and unclad except for an animal skin, much of his time was spent in meditation, seated quietly on another animal skin. The cold distracted him, and his teacher advised him to concentrate on *warmth* and to visualize himself in extremely high temperature situations, telling him that then he would acquire bodily warmth. He reported gradual success *after about six months of practice*. Later he found that in a moderate climate the same practices produced not only increased sensations of warmth but perspiration. We suggest that this result represents a conditioned ANS response pattern to visual imagery based on reaction to the ambient

[3] Source: Electro-Medical Engineering Co., 2317-A West Olive Avenue, Burbank, California.

TABLE 1

EXPERIMENTS WITH SWAMI VIGYANANAND ON VOLUNTARY SWEATING

	Time	FoR	PR	FoT	FiT	ArmT	SBP	DBP
Initial Values		93	148	34.4	24.4			
First Attempt (March 8; Room temperature 21.7° C.)	3:45	93	132	34.7	24.8			
	3:50	40	92	34.9	23.7			
	3:52	20*	82	34.3	23.5			
	3:55	32	100	33.8	23.2			
Second Attempt	4:00	39	100	33.6	22.9			
	4:03	22*	92	34.4	23.8			
	4:05	40	105	33.6	22.5			
Initial Values		38	142	35.1	35.2	34.0	116	66
First Attempt (March 19; Room temperature 28.5° C.)	3:15	30	152	35.9	34.8	34.3		
	3:17	20*	122	36.0	34.0	34.5		
	3:20						150	70
	3:25						124	80
Rest	3:35	25	133					
	4:10	25	133	35.0	35.5	34.0	132	80
	4:11	Experiment terminated at request of S						

FoR Forehead skin resistance in K ohms
PR Palm-palm skin resistance in K ohms (on second day, palm-upper arm)
FoT Forehead skin temperature in °C
FiT Finger skin temperature in °C
ArmT Arm skin temperature (at biceps) in °C
SBP Systolic blood pressure in mm. Hg.
DBP Diastolic blood pressure in mm. Hg.
* Marked perspiration visible on forehead

temperature in the caves, which we estimate to have approached 0° C. on occasion.

We found no one with voluntary control over internal sphincters of anus or urethra, but have no reason to doubt that it is possible. We did find those who employ a catheter or other form of tube and, with its help, are able to draw up water into the bladder or lower bowel. They claim that after long practice the artificial aid can be abandoned. One other subject claimed to be able to defecate at will if any fecal matter was present. He employed a particular position and movement of the tongue to aid in this control. Again the control had only been gained after long practice. In all of these forms of control it seems reasonable to assume that conditioning had occurred.

Another subject regurgitated at will, but he professed to no special practice. Instead he stated that as a young man he discovered he could do this, and used it occasionally as a cleansing device. Motion picture records suggest that the process involves a special use of striated muscles in the common yogic exercise previously referred to as *uddiyana*. It involves raising of the diaphragm and inward distension of the abdomen. Here the maneuver was accomplished suddenly and forcefully.

Our experiments with four subjects who claimed to control the heart and pulse are reported separately (Wenger, Bagchi & Anand). The heart did not stop in any experiment. For three subjects there were brief periods during which we could detect no heart sounds with a stethoscope and the palpable radial pulse disappeared, but EKG records showed the heart to be in continuous action, and plethysmographic records from a finger showed that the pulse did not disappear. The fourth subject claimed only to slow the heart; and this he demonstrated on three different days. The mechanism was not discovered. It is presumed to involve increased vagal tone, and is associated with breath retention, arching of the back, and considerable tension of abdominal and neck muscles. Further research is being

conducted with this subject (Kuvalayananda).

Bradycardia did not occur in our other subjects, but they also employed intervening striated-muscle action in a maneuver similar to that described long ago by Valsalva. During strong abdominal contraction and breath arrest with closed glottis there apparently occurs an increase in intrathoracic pressure to the point where venous return to the heart is markedly reduced. The heart continues to contract but it pumps little blood. Heart sounds and palpable pulse thus may seem to disappear. Moreover, during breath retention, there is a deviation in position of the heart so that potentials in lead I are decreased, but those in lead III are increased. Dr. Brosse's (1946) record of "heart control" involved lead I only. Had she measured any other lead her conclusions concerning voluntary control of the heart probably would have been negative, as are ours.

Changes during relaxation and meditation

Recordings were made as 14 subjects engaged in meditation in 25 sessions. The median time per session was 49 minutes; the range was 14–148 minutes. An initial over-all view of the results has been presented (Bagchi & Wenger, 1957, 1959). Here a few selected comparisons are reported without any statistical consideration or suggestion of definite trends.

Figure 1 shows physiological data during relaxation and during meditation for four students of Yoga.[4] Greater sympathetic NS activity is seen during meditation than during relaxation. In the latter state heart rate is slower, finger temperature is higher, palm-palm skin conductance is less, indicating less palmar sweating, and both systolic and diastolic blood pressure are lower. Only for respiration do we find greater activity during relaxation. The obtained differences are attributable in part to posture. The subject reclines in the *asana* known as *shavasana* for relaxation; during meditation he is seated with legs crossed.

The bottom of Figure 1 shows few marked changes in autonomic functions during meditation in five yogis, all older than the students, and all longer in practice. Concomitant studies of the EEG also showed no definite changes (Bagchi & Wenger, 1957, 1959). It should be pointed out, however, that some of the subjects seemed to be bothered by the apparatus we attached to them during our meditation experiments, and none professed to having achieved, during our recordings, the deepest meditational states they had experienced. It also should be noted that individual subjects occasionally demonstrated marked increases or decreases in most of the autonomically innervated functions we recorded (e.g., slow breathing 4–7 per minute, see Bagchi & Wenger, 1957, 1959). Only repeated investigations with the same subject can determine whether these changes are correlated with his meditational experiences.

It is of interest to note that the older yogis demonstrated faster heart rates during meditation than did the students, as well as lower finger temperature, greater palmar conductance, and higher blood pressure. All of the differences suggest that, for these Yogis, the process is an active one, not a passive relaxed contemplation; although some muscular relaxation is indicated by the decreases in palmar skin conductance—a finding which was typical. If such an observation is valid, then perhaps part of the differences in Figure 1 are also attributable to the meditative process. The slower respiration rate during meditation is not a contradiction here, nor is the apparently greater finger pulse volume for these yogis. The latter measure lacked absolute calibration and is valid for within-group comparison only; and respiration often becomes slower and more regular even during unpracticed concentration.

The data on palmar skin conductance deserve particular mention. The expected trend during muscular relaxation is a decrease (Wenger & Irwin, 1936). That the decrease is so slight in the upper part of Figure 1 is

[4] The data shown in this portion of Fig. 1, and those in Figs. 3 and 4, were obtained from students of Swami Kuvalayananda at Lonavla and represent experiments on three days for each subject. Although eight students participated in these and other experiments, data for some were not complete enough to be included in these analyses.

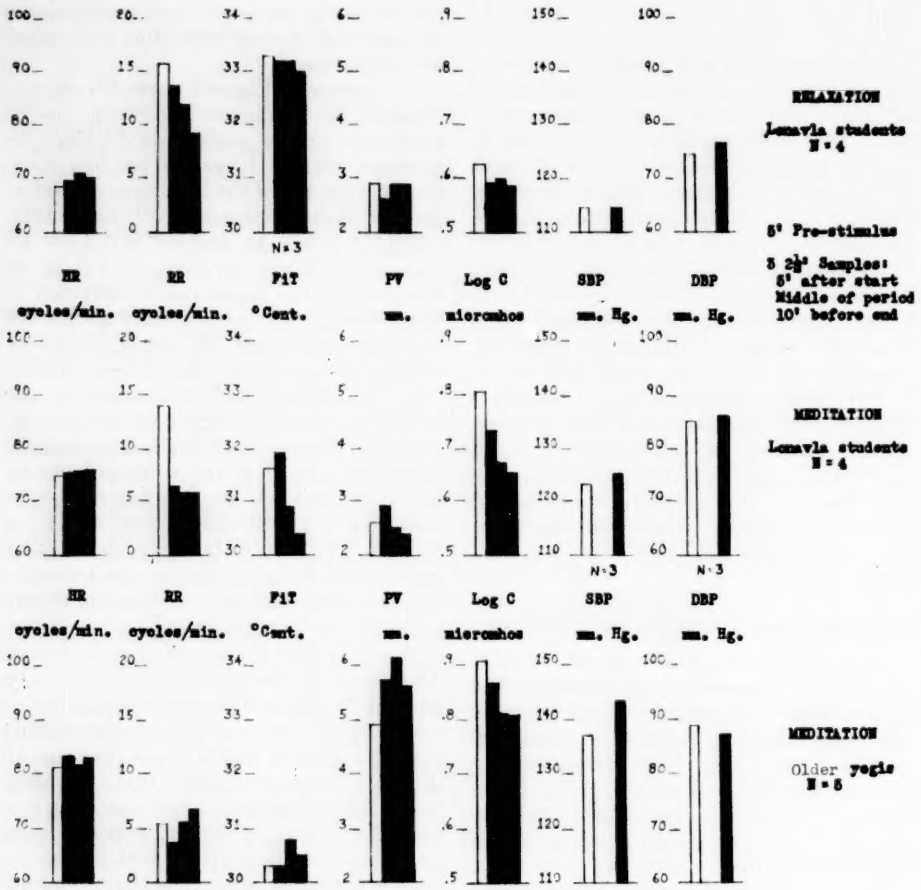

FIG. 1. Physiologic Recordings during Relaxation and Meditation.[a]

HR = Heart rate
RR = Respiration rate
FiT = Finger temperature
FV = Finger pulse volume
Log C = Palm-palm log conductance
SBP = Systolic blood pressure
DBP = Diastolic blood pressure

[a] The authors wish to acknowledge the assistance of Mrs. Susan Baker and Dr. Thomas D. Cullen in the analyses of data presented in this article. They also wish to point out that the samples of data selected for illustration in Figures 3 and 4 were chosen as representative of the condition or exercise under study. Obviously the figures do not reflect individual variations or trends. That is particularly true of the bottom part of Figure 1. With few subjects and great individual variability, no other group treatment of the data seemed appropriate.

The histograms in the figures represent means for subjects and conditions as indicated in the right-hand margins; and the portions of each histogram correspond to the sequence of samples as described. Incomplete histograms indicate discontinuous measurement, but spatial arrangement shows the available samples.

probably due to the fact that considerable relaxation occurred in our reclining subjects during the attachment of equipment and during the control period *before* the instruction to relax was actually given. On the other hand, for the measurements during meditation, the subjects were seated in crosslegged posture with eyes open until they were asked to begin meditation.

For several subjects, slow oscillatory skin resistance waves appeared in the later part of the recordings. It is the most unusual physiological change we found during our experiments on meditation. A similar phenomenon has been reported by Brosse (1950); and it probably deserves further investigation. It should be remembered, however, that such waves may be caused by apparatus artifacts or by overt or covert changes in body position. If these skin resistance changes are to be associated with meditation *per se*, there must be a careful inspection of equipment, and muscle potentials must be measured. The phenomenon occurred rarely in our work, and we were not prepared for it. Perhaps others may now look for it.

Respiratory exercises

The next figures deal with respiratory exercises, or *pranayama*. Figure 2 shows the respiratory patterns in *bhastrika* and in *ujjayi*. In the former there is a series of short abdominal inspirations and expirations followed by a prolonged retention of air. In the latter there are long periods of inspiration, retention, and expulsion. Particular benefits are claimed for each.

Figure 3 shows that physiological differences between the two measured for the same five subjects on different days are slight. With larger groups statistically significant differences may be found, but from our data it is difficult to believe that these two respiratory exercises could produce marked differential changes in behavior. It seems probable, however, that any experiential changes attributable to either are not due to hyperventilation. Results from an experiment with the latter are shown in the lower part of the figure. Here the pattern appears to be quite different, at least in heart rate, finger temperature, and pulse volume.

Figure 4 shows data obtained during another form of *pranayama*, which also was followed by hyperventilation. This breathing

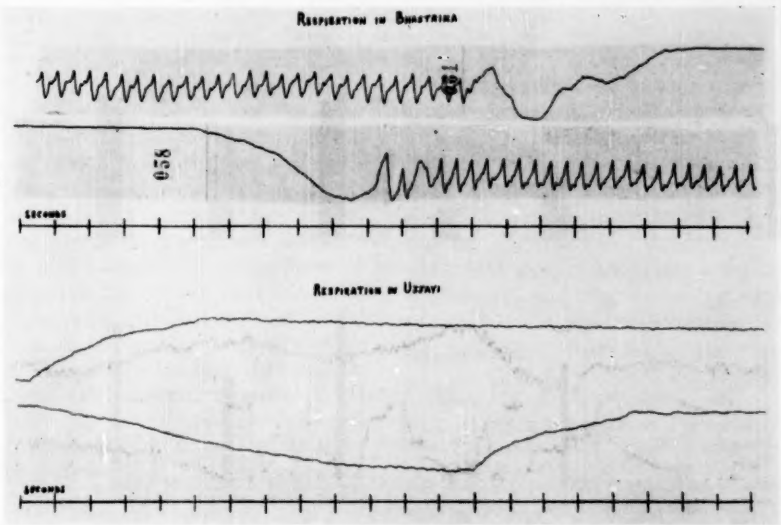

FIG. 2. Respiration Patterns in Two Forms of *Pranayama*.

exercise, known as *kapalabhati*, is the shallow, rapid abdominal inhalation and exhalation that constitutes a part of *bhastrika*. It sometimes is practiced alone. We thought it might produce the hyperventilation syndrome, and tested this hypothesis. Again the patterns proved to be different.

We have but scratched the surface of a fascinating research area, one that we believe should be thoroughly explored. In particular subjects, especially more experienced yogis, altered breathing has produced bidirectional changes in every autonomic variable we measured. Moreover, one of the Yogic methods of relaxation involves simply a concentration on maintaining steady respiration. Certainly one goal of future research in this area should be to learn more about the effects of respiration on behavior.

Studies of autonomic balance

While waiting for promised subjects at New Delhi we engaged in work we had not planned. We conducted measurements of resting autonomic functions on "beginning" and "advanced" students of Pandit Prakash Dev at the Yogashram there, to afford a preliminary test of the claim that practice in *asana* and *pranayama* are beneficial to mental and physical health. If such be the case we assumed that autonomic nervous system functions would reflect such benefits by upward shifts in scores of autonomic balance (autonomic factor estimates), and shifts in individual variables indicative of decreased sympathetic NS activity. The rationale for this assumption, and a description of the testing methods, has been presented elsewhere (Wenger, 1948), along with

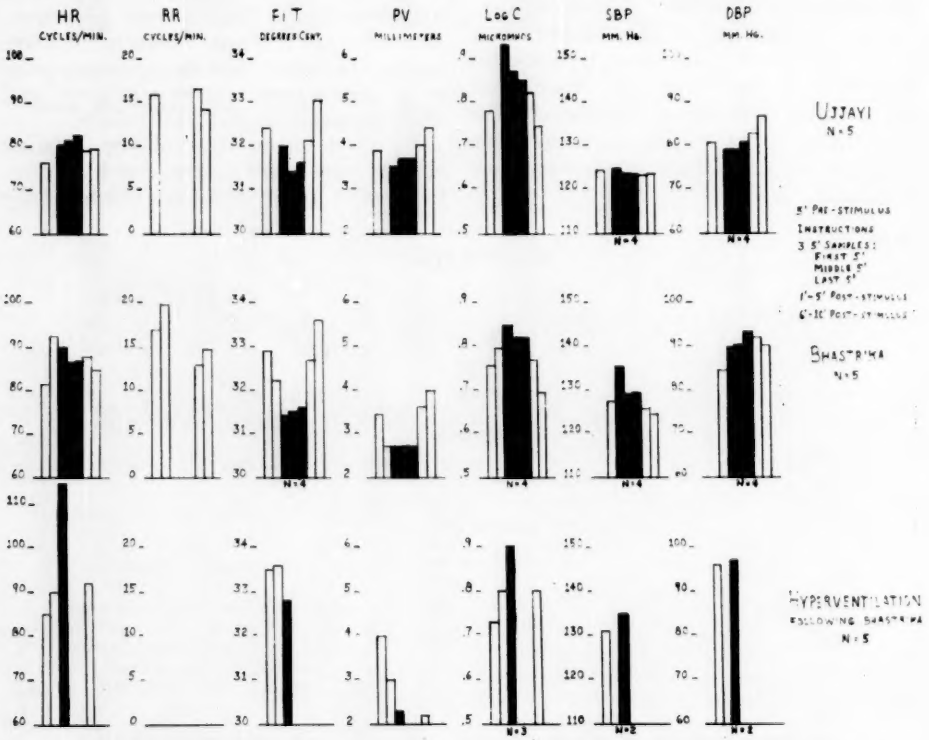

FIG. 3. Physiologic Recordings during Two Forms of *Pranayama* and during Hyperventilation. (Variables as in Fig. 1)

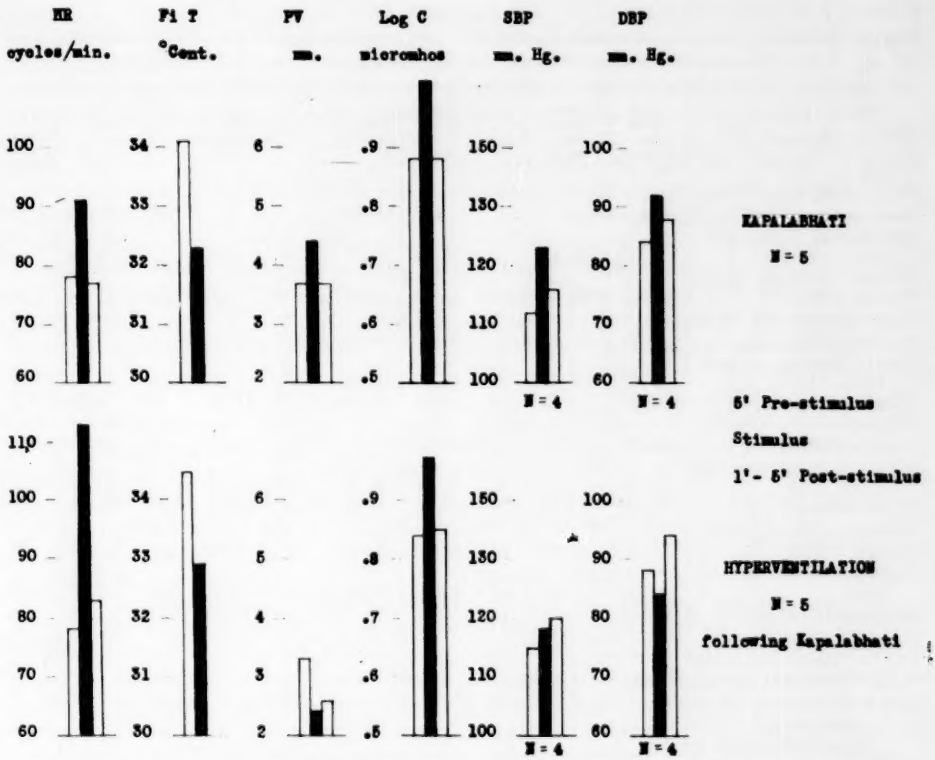

FIG. 4. Physiologic Recordings during One Form of *Pranayama* and during Hyperventilation. (Variables as in Fig. 1)

"normative" data for 488 young healthy males who were candidates for air-crew positions in the U. S. Air Force.

Later we obtained similar data from students of Swami Kuvalayananda at Lonavla. The results are shown in Table 2 in standard score form based on means and standard deviations of the Air Force data. Since for the latter group the transformed mean for each discrete variable is 50 and the standard deviation is 10, the table also provides an immediate comparison between the students of Yoga and the reference group of U. S. males. In the last row of the table showing mean autonomic factor scores, i.e. $\bar{A}$ a derived composite measure, the reference mean is 69 and the standard deviation is 8.[5] Some variables are reflected so that scores below the reference mean indicate apparent dominance of the sympathetic system (SNS) (i.e., high SNS or low PNS activity), while those above it indicate either reduced SNS function or increased parasympathetic (PNS) function.

The first part of the table compares the "beginning" and "advanced" students at New Delhi. The terms are relative. Some of the "beginners" had had as much as three months' practice; only two of the "ad-

[5] The equation for deriving the scores of the autonomic factor utilizes standard scores for the first seven variables shown in the table (Wenger, 1948). Because of lack of suitable equipment it was impossible to measure salivary output in the New Delhi subjects; therefore, for them the reference mean of 50 was used in the equation. Had the true data been available their composite scores probably would have been slightly lower.

TABLE 2

MEAN STANDARD SCORES FOR PHYSIOLOGIC TESTS OF YOGI STUDENTS WITH *t* TESTS BETWEEN NEW DELHI AND LONAVLA MEANS, AND BETWEEN INDIAN AND U. S. MEANS

Variable	New Delhi Beginners (n = 9)	New Delhi Advanced (n = 8)	t	New Delhi Combined (n = 17)	Lonavla Students (n = 7)	t	New Delhi Combined minus U. S.	t	Lonavla minus U. S.	t
Salivary output (SO)	✱	✱		✱	38.67				−11.33	2.76†
Sublingual temperature$_x$ (ST)	53.33	52.43	.17	52.94	47.71	1.13	2.94	1.15	−2.29	.60
Stand. palmar conduct.$_x$ (PC)	44.33	50.50	1.13	47.24	62.14	3.33†	−2.76	1.11	12.14	3.20†
Volar forearm conductance$_x$ (VC)	31.78	30.50	.17	31.18	50.29	3.17†	−18.82	7.47‡	.29	.08
Log palmar cond. change (LCC)	49.67	48.50	.30	49.12	78.57	7.38‡	−.88	.36	28.57	7.48‡
Heart period (HP)	37.11	35.75	.28	36.47	50.00	3.43†	−13.53	5.48‡	0.00	—
Diastolic blood pressure$_x$ (DBP)	35.22	35.88	.16	35.53	39.43	1.07	−14.47	5.88‡	−10.57	2.78†
Finger temperature (FiT)	13.60	3.63	.91	7.46	28.14	2.31*	−42.54	14.67‡	−21.86	5.65‡
Pulse pressure (PP)	35.44	35.00	.22	35.24	40.57	1.79	−14.76	6.05†	−9.43	2.47*
Systolic blood pressure$_x$ (SBP)	51.89	47.88	.91	50.00	49.86	.03	0.00	—	−.14	.04
Respiration period (RP)	41.33	46.00	1.45	43.53	47.43	1.11	−6.47	2.64†	−2.57	.67
Autonomic factor (Ā)	58.17	57.72	.11	57.96	70.79	3.47†	−11.04	5.58‡	1.79	.59

$_x$ = Reflected variable
✱ = Variable not measured
* Significant at .05 level
† Significant at .01 level
‡ Significant at .001 level

vanced" group had been practicing more than one year, and most for about six months, not always regularly. Although no statistically significant differences were found between means for the two groups, we attach no importance to the results. The groups were small, and not sufficiently dissimilar in practice.[6]

Since the New Delhi groups did not differ significantly, their data were combined for comparison with those obtained later at the Lonavla Ashram. There, eight full-time students of Hatha Yoga were available, seven of whom had engaged in almost daily practice for two to seven years. One was a neophyte, so his data were eliminated. The comparison appears in the second part of Table 2. It may be seen that six of the eleven variables show statistically significant differences, all in the predicted direction of higher mean scores for the more practiced students, and that the autonomic factor score not only shows a significant difference from that of the New Delhi group, but is slightly higher than the mean (i.e., 69) for the U. S. reference group.

Although it may be tempting to argue that this comparison demonstrates one aspect of the beneficial results of Yogic practices, we believe the results should be cautiously interpreted. The two groups differed in other respects than amount of practice. Most of the New Delhi group enrolled for training for reasons of physical culture, and few practiced regularly, or for more than one or two hours per day. Most of the Lonavla group were dedicated students of the spiritual

[6] Our apparatus was donated to the Indian Council of Medical Research through mutual agreement by the Rockefeller Foundation and the University of California. It is now being utilized by Dr. B. K. Anand and Dr. Gulzar Singh of the All-India Institute of Medical Sciences at New Delhi. One of their objectives is to improve and extend this type of comparison.

aspects of Yoga. Most practiced daily for two hours or more and all also engaged regularly in meditation. Controlled longitudinal research perhaps offers the best approach for investigating the effects of Yogic practices. In the meantime, the present results are suggestive only.

They do afford other points of interest. As indicated in the third and fourth parts of Table 2, both Indian groups show a number of significant differences in means from the U. S. reference data. For the New Delhi group seven of the eleven variables, including the mean autonomic factor score, differ in the same direction at or beyond the .01 level of confidence. There seems no doubt that this group de onstrated an apparent dominance of SNS function. Although six of twelve means for the Lonavla group also differ significantly from the reference data, the directions of differences are mixed, and the composite autonomic factor score is not among them.

Three variables for both Indian groups differ significantly in the same direction and indicate that both groups demonstrate lower finger temperatures, higher diastolic blood pressures, and lower pulse pressures than U. S males—all suggestive of greater peripheral vasoconstriction. These findings have led us to urge the collection of normative data from a larger Indian group. With the exception of salivary output, for which the data were incomplete, the other differences suggest group rather than national characteristics.

For the New Delhi group the higher forearm conductance (greater sweating), the faster heart and respiration rates (shorter periods), and the lower autonomic factor scores may reflect the mental and physical health problems which led many of the students to enroll for training.

Of most interest perhaps are the differences shown by the Lonavla group in electrical conductance from palmar skin areas. The first (PC), employed as an indirect measure of palmar sweating while standing in a relaxed posture for two minutes, will show lower readings in individuals who progressively relax even while standing. The second (LCC) represents a more direct measure of relaxability. It measures the decrease in palmar conductance from a position of strain to the lowest conductance reached during a 12-minute period of reclining under the instruction, "Relax as much as possible, but don't go to sleep." A log transformation is employed to correct for base line effects. The results indicate that the Lonavla students demonstrated high relaxability in both measures, which is not surprising since they were well practiced in voluntary muscular relaxation. In our opinion the results for these two variables do constitute evidence for one beneficial effect of Yogic training, especially if the New Delhi data are also considered. In view of the other data from that group, low standard scores also might have been expected in these two variables. Instead, the group means approximate those for the U. S. reference group. It seems possible that the New Delhi students already were learning to relax the skeletal musculature.

It should be noted that the differences shown in Table 2 probably cannot be attributed to ambient temperature effects. First, our schedule for work in different locations was arranged to escape extreme temperatures and to permit measurement at ambient temperatures comparable to those obtaining for work in this country. Our goal was to confine our work periods to a temperature range of 72°–80° F., and rarely did we depart from it. Second, those variables that required correction for room temperature (also for time of day) as determined by previous work (Wenger, 1948) were corrected. Both the finger temperature and forearm conductance data in Table 2 include corrections for both temperature and time of day, while corrections for time of day only were applied to sublingual temperature and systolic blood pressure. Temperature changes must be taken into account, of course, in any research on ANS functions in India, as Table 1 of this report well illustrates. (See in particular the differences in forehead skin resistance and in finger temperature on the two days of measurement.)

Miscellaneous observations

By way of conclusion we might mention that we found no fire-walkers; none who stuck needles into their bodies with or with-

out bloodshed; and none who lay on beds of nails, except one exhibitionist. For the latter the task was easy. By practice he had learned to relax the muscles of the back, and the nails were close together and not very sharp. One of those who worked with us had engaged in a number of pit burials, but he would not agree to such an experiment with us.[7]

We have no doubt that fire-walking occurs. In south India and in other locations it seems to be a common practice in connection with certain religious or other celebrations. We are informed, however, that the coals are well covered with ashes and that the "walk" is more like a "run." Moreover, the participants are not yogis; they are the young men of the villages. And, since they have seldom or never worn shoes, the epidermis of the soles of their feet is reported to us as being from ⅛″ to ¼″ in thickness. There may be fire-walkers with tender feet, but we have not seen them or heard about them in India.

When people ask us about our experiences in India, they frequently mention "levitation." We found no direct evidence of this purported phenomenon. We did find individuals who believed in it and who knew people who had seen it. Although the investigation of levitation was not one of our objectives, one cannot long discuss Yogic phenomena in India (or anywhere) and escape it. Let it be recorded that the closest we came to levitation was to meet a man who had treated a scalp-wound for a yogi who claimed he had been levitated involuntarily.

Finally we wish to place on record that we have met many dedicated yogis who have described experiences to us that few Western scientists have heard of and none has investigated. It is possible that the mere presence of a foreigner precludes optimum results. Perhaps this is a field of human experience that Indian scientists can best explore.

[7] Since our departure this man has served in pit-burial experiments for Dr. B. K. Anand and Dr. Gulzar Singh of the All-India Institute of Medical Sciences at New Delhi. Another yogi whom we attempted to find had engaged in such an experiment in 1956 for Dr. Gowindaswamy and his associates at the All-India Institute of Mental Health at Bangalore. This man has recently been a resident at Kaivalyadhama, the Ashram at which we worked in Lonavla. There he has engaged in other pit-burial experiments under the supervision of Swami Kuvalayananda, Dr. S. L. Vinekar, and their associates. Reports of these experiments should soon be available.

CONCLUSIONS

Within the limits of our experience the following conclusions were drawn:

(a) Direct voluntary control of autonomic functions is probably rare among yogis. When such control is claimed, intervening voluntary mechanisms usually are employed.

(b) Marked changes in autonomic functions occur during some Yogic exercises. Their immediate and long-term effects deserve further investigation.

(c) Possible racial differences in autonomic nervous system functions have been reported.

(d) Possible beneficial results of Yogic practices have been reported.

(e) Possible physiologic changes associated with meditation experiences need further study.

REFERENCES

Bagchi, B. K. & Wenger, M. A. Electro-physiological correlates of some yogi exercises. *The First International Congress of Neurological Sciences*, Brussels, 1957. Vol. III. *EEG clinical neurophysiology and epilepsy*. Pergamon Press: London, 1959, p. 132–149.

Brosse, Thérèse. A psycho-physiological study. *Main Currents in Modern Thought*, 1946, 4, 77–84.

Brosse, Thérèse. Altruism and creativity as biological factors of human evolution. In P. A. Sorokin (Ed.), *Explorations in altruistic love and behavior*. Boston: Beacon Press, 1950, p. 118–145.

Kuvalayananda, Swami. Unpublished research. Kaivalyadhama, Lonavla, Bombay State, India.

Wenger, M. A. Studies of autonomic balance in Army Air Forces personnel. *Comp. Psychol. Monog.* 1948, 19, No. 4.

Wenger, M. A. & Bagchi, B. K. A report on psychophysiological investigations in India. (Mimeographed manuscript for limited circulation.)

Wenger, M. A., Bagchi, B. K., & Anand, B. K. Experiments in India on voluntary control of the heart and pulse. *Circulation*, in press.

Wenger, M. A., Engel, B. T., & Clemens, T. L.

Studies of autonomic response patterns: rationale and methods. *Behav. Sci.*, 1957, 2, 216–221.

Wenger, M. A., & Irwin, O. C. Fluctuations in skin resistance in infants and adults and their relation to muscular processes. *Univ. Iowa Stud. Child Welf.* III. *Studies in infant behavior*. 1936, 12, No. 1, 7–90.

For additional references on Yoga, see Bagchi & Wenger, 1957, 1959.

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Let each one examine his thoughts, and he will find them all occupied with the past and the future. We scarcely ever think of the present; and if we think of it, it is only to take light from it to arrange the future. The present is never our end. The past and the present are our means; the future alone is our end. So we never live, but we hope to live; and, as we are always preparing to be happy, it is inevitable we should never be so.

BLAISE PASCAL, *Pensees*

CRITIQUE AND COMMENT

Students of international relations are currently engaged in considerable controversy as to the theoretical and methodological directions which the field ought to take. This paper adds some fuel to the fire in discussing the uses and limits of sociology, psychology, and anthropology as they might apply to the development of international relations theory.

THE RELEVANCE OF THE BEHAVIORAL SCIENCES TO THE STUDY OF INTERNATIONAL RELATIONS

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INTRODUCTION[1]

THE past few years have witnessed a rather remarkable intellectual ferment among students of international political relations. Since the post-World War I period, when the subject first began to receive serious academic attention and to find its way into the regular college curriculum, we have tended to restrict ourselves to the description of institutions and to the chronological exegesis of past events, occasionally venturing into the quagmire of contemporary policy problems.[2] But in recent years, there has been a growing dissatisfaction with the manner in which our subject was approached, studied, and taught; and this dissatisfaction has produced an impressive array of discussions, on the state of the discipline in general and on the need for theory in particular.[3]

Despite this encouraging self-consciousness, however, there has been little formal attention to the potential role of our sister social sciences in the development of international relations theory; especially has this been true regarding the behavioral social sciences—anthropology, psychology, and sociology.[4] Outside of the articles by Deutsch (1958) and Guetzkow (1958), we have seen almost no serious, affirmative discussion of the relevance of these three disciplines to our own, and neither of these articles was intended to be a formal and systematic treatment of this possible relevance. Given the impressive development of these disciplines and the reappraisals now occurring within the international relations field, this seems to be a particularly propitious moment for attempting such a survey. The purpose of this article, therefore, is to discuss—albeit briefly and sometimes superficially—some possible areas in which these disciplines may be expected to make a useful contribution to the development of theory in international political relations. I have divided these potential applications into three categories: the methods, the findings, and the concepts

[1] This is an expanded version of a paper presented to the University of Michigan Sociology Colloquium on December 16, 1959. The author is particularly indebted to the Ford Foundation, whose International Relations Training Fellowship enabled him to spend the academic year 1957–1958 as a Visiting Fellow in the Department of Social Relations at Harvard University. His modest understanding of the behavioral sciences is in large measure due to the cordiality and interest with which the members of that Department received and instructed a disciplinary outsider.

[2] For the growth and development of the discipline, see: Ware (1939); Zimmern (1939); Fox (1949); and Thompson (1952).

[3] Some manifestations of this concern are two recent collections of readings: Fox (1959) and Hoffmann (1960). For a critical review of these, see Singer (1960).

[4] For an excellent treatment of the behavioral sciences more broadly conceived, see Miller (1955). The behavioral social sciences have been defined in equally broad terms to include history, geography, economics, and politics; see Harvard University (1954) and University of North Carolina (1954).

of the behavioral sciences. Let us begin, however, by discussing the role of theory itself and its value to the state of the discipline.

THE ROLE OF THEORY

The major role of theory[5] in any discipline is the inescapable one of permitting the systematic ordering of data, the absence of which deprives that subject of the right to be considered a scholarly discipline. To employ a crude analogy, one might liken the welter of data bearing on international relations to the inchoate mass of detail appearing on an aerial photograph of a wide and varied terrain. To the lay eye, the photograph is essentially meaningless, but the trained photo-interpreter employing systematic techniques and carefully formulated categories is quickly able to discern roads, hills, swamps, dwellings, and the like. Similarly, little significance or coherence can be made of the tremendous body of fact which spreads over the space and time of international political relations without some conceptual framework by which these facts may be ordered, interpreted, and assigned meaning. In the absence of such a framework, there is little possibility of distinguishing between the trivial and the crucial, the unique and the uniform, the cause and the effect. As one student expressed it, a concern for theory may produce "modes of analysis that can use the events of widely separated periods despite the fact that their only bond is their relationship to a principle and not proximity in time" (Thompson, 1955, p. 734).

The second major virtue of a body of theory is that it encourages an articulation of assumptions. One of the most striking aspects of almost every text in the field today is the near absence of any spelling out of the author's assumptions. Does he see man as essentially evil, basically good, or morally neutral? Is his image of the international system characterized as normally peaceful, or conflictful? Is his major focus on the larger system, the state, the national elite, the individual, or the environment?[6] Does he view the state as basically sovereign and autonomous, or as the hapless object of external forces, buffeted about by historical inevitability? These assumptions are crucial to an understanding of the scholar's treatise, yet they are seldom spelled out. And if one struggles bravely to "tease out" those "bootlegged" assumptions, he later discovers that they are adhered to only intermittently and when it suits the author's purpose; such are the virtues of "invisible theory." The development of a body of coherent theory in international politics, even if at the lower-middle ranges, would sharply reduce the incidence of such sins.

Third, and by definition, the use of a theoretical framework would require the observer to make clear and explicit the criteria by which he assigns significance to his data. It would inhibit, to borrow a telling phrase, the practice in which "history is . . . ransacked for confirmation of a postulate," where "facts that do not fit are left out . . ." (Hoffman, 1959, p. 355). The scholar would be compelled to spell out his reasons for selecting certain events and omitting others, and for emphasizing one set of variables and ignoring another.

Fourth, a focus on theory could well stimulate a self-generating process out of which might develop a more thoroughly validated or higher-level set of generalizations. Because the use of a theoretical framework requires that we specify and define accurately our variables and param-

[5] Though I would prefer to avoid the pitfalls involved and the controversy that is bound to follow, several readers of an earlier draft challenged me to spell out what I mean by theory in this context. A theory of international relations would be an orderly and systematic collection of consistent generalizations describing and explaining the behavior of nations in a variety of situations, and the reciprocal relation of such behavior to the characteristics of the larger political system within which the nations interact. Such generalizations might vary widely from the particular to the general, may apply to a specific era and region, or to several, and could focus primarily upon either the actors or the system. But they must be susceptible of empirical disconfirmation, unambiguous as to referents, and clearly differentiate between the descriptive-explanatory and the normative.

[6] For a discussion of this question of focus, see Singer (1959, 1961) and Waltz (1959).

eters, it would permit replication of one man's studies by others during the early stages of development of a body of theory. If several scholars employ the same variables, assign them the same meaning, and reach the same conclusion, that conclusion is more likely to be valid than if it represents the unique result of a single observer's discursive appraisal of a discrete case.[7] Such is the building process in a body of knowledge.

Finally, a word may be in order regarding the relevance of theory for the *practitioner* in the field. The setting in which foreign policy decisions are taken has become increasingly complex and diminishingly vulnerable to control during the twentieth century. As a result, if the statesman is to avoid that ultimate blunder which could plunge the world into nuclear obliteration, he must be better able to predict the behavior of his opposite numbers, and at the same time comprehend the nature of the limitations under which all must operate. In the absence of a fairly well-developed and valid set of propositions concerning the system and its actors, he is much more likely to err. If the tenuous stability of the modern power balance is to be preserved and increased, there needs to be a closer approximation to rational behavior based on what the game theorist (Kaplan, 1957, pp. 172–173) refers to as "complete information."[8]

In summary, the need for theory cannot be overestimated. If the study of international politics is to advance from its current archaic state, in which there is much opinion and little knowledge, and in which one man's assertion is as "valid" as the other's, we must address ourselves self-consciously and deliberately to the development of a coherent body of theory.[9] Can the other social sciences, particularly the behavioral sciences, help us in this endeavor? Perhaps the most fruitful way of exploring these possibilities lies in considering the three attributes which characterize any scholarly discipline: methodology, findings, and concepts. These will be discussed in turn.

THE METHOD OF THE BEHAVIORAL SCIENCES

In considering the possible usefulness of behavioral science methodology to the student of international relations, one is, of course, merely referring to the general notion of scientific method, applied to the study of man as a social creature. Admittedly, there are a great many philosophical arguments leveled against the belief that man and his institutions are even susceptible to scientific study, but this debate has been repeated frequently enough to be dispensed with here. Rather, having postulated the relevance of scientific method to inquiries concerning individual and aggregate behavior, our task is that of delineating the major characteristics of such method as it applies to international relations.

One of the most fundamental properties of this scientific method is its acceptance of probabilism—that almost no generalized proposition, no matter how frequently put to the test, represents a universal and absolute truth. To nearly every statement of generalization, there are bound to be exceptions. Consequently, all predictions become essentially probabilistic; and to assert, for example, that states *always* assign a higher priority to the pursuit of external power than to the material needs of the citizenry when the two are in conflict, is to exceed the bounds of scientific propriety. Rather, by qualifying the proposition, one might legitimately speak of tendencies under specified conditions. But even then, one must still emphasize that the observed tendency is only accurate on the basis of evidence then available; that is, the

[7] The psychologists who bemoan the redundancy of certain oft-repeated experiments would better remain silently appreciative of their discipline's capacity for replication. The political scientist can only gaze in envy!

[8] For an excellent treatment of the use of theory in policy making, see Fox (1959, Ch. 3).

[9] I have eschewed for the moment the temptation to delineate exactly what this theory ought to comprise, what its level of abstraction ought to be, or which end of the inductive-deductive continuum might best serve as a point of departure. For a provocative, if not wholly persuasive, discussion of such points, see Hoffmann (1959).

uncovering of additional data may readily refute or modify the proposition as articulated. Implicit in this relativistic stance is the awareness that all that a scholar has achieved in a lifetime may be proved erroneous; and moreover, each scholar must constantly seek to disprove or modify his own research conclusions. The dogmatic assurance of those who discover their "truth" in natural law or divine revelation is not afforded to the empiricist, and the discomfort of such an awareness has certainly helped to inhibit the development of a scientific outlook among political scientists.[10]

A second major characteristic of the behavioral scientist's approach is his continuing preoccupation with the inductive-deductive continuum. He must constantly be deciding whether the main task of the moment is that of data-gathering or systematization, or, to put it in the terms of John Wisdom (1952), whether to pursue discovery by investigation or discovery by reflection. Intimately involved in this question is whether (the scientist having selected for the moment the latter approach) the hypothesized statement of relationships is operational, i.e., susceptible of demonstration via empirical observation. Furthermore, he must constantly attempt to bridge the gap between discrete fact and high-order abstraction, eschewing the easy path of either extreme. Thus, the student of international politics who rejects the pursuit of theory because "we have much more data-gathering to do first" is as guilty of a non-scientific outlook as the one who neither engages in data-collection himself nor examines that of others, but merely spins vast and speculative webs in the name of theory-building. As one political scientist (Apter, 1957, p. 747) has put it, the problem is one of avoiding "the effects of descriptive detail as a substitute for theory . . . without losing the advantages . . . of the specialized knowledge and lore so laboriously acquired."

It might also be appropriate here, despite the danger of belaboring the obvious, to reiterate briefly some of the other attitudes common to the behavioral sciences that are so often ignored by the political scientist concerned with inter-nation relations. One is the concern for replicability as a means of validation by which low-order or middle-order statements may be converted into empirical generalizations of wider scope. And out of this flows, in turn, the emphasis on specificity of variables—the attempt to analyze the variable into each of its dimensions. For example, were students of international politics to reduce the oft-used variable of "danger" to its two dimensions of utility (or disutility) and probability (subjective or objective), they would be considerably more justified in speaking of one outcome or policy as being more or less "dangerous" than another. (And, parenthetically, it would compel an awareness of the extreme subjectivity of such a concept. See Thrall, et al. [1954].) But as long as we believe that such phrases as dependent variable, parameter, selective distortion, social distance, drive-reduction, cathexis, role-mediation, reaction-formation, etc., are mere jargon, we will stumble along, using such important but vague, all-inclusive, multidimensional concepts as tension, nationalism, international atmosphere, ideology, functionalism, etc., in their present imprecise and confusing fashion. And, as a consequence, many of us will investigate different situations in which these are our major variables, will assign them rather different meanings, and will never achieve any professional consensus as to their precise dimensions. Without such a consensus of precision and specificity, replication (and hence validation) must remain only a faint and future hope.[11]

Research design

Before turning from these rather general attributes of behavioral science methodology

[10] Such a position does not, however, require that his *values* be empirically verifiable; it only requires that we distinguish between what we believe and *prefer* and what we think we *know*.

[11] Lest I give the impression that there is *no* concern for the specificity of variables among international relations scholars, let me note several impressive exceptions, stemming primarily from the Princeton Center for Research in World Political Institutions: Van Wagenen (1955) and K. W. Deutsch et al. (1957); also see Guetzkow (1956), H. and M. Sprout (1956), and K. W. Deutsch (1954).

to some of the more specific methods and techniques employed in these disciplines, let us refer briefly to the idea of research design, which is in a sense the bridge between the general posture and the specific techniques of the behavioral scientist.[12] The traditionally-trained political scientist or historian normally decides that it would be "interesting" to "know more" about a specific event, sequence of events, or chronological era, and then proceeds to pore over the literature in a somewhat random fashion, occasionally noting certain general trends or configurations; these he often elevates to the heady status of "theory." The more systematically trained social scientist (especially in psychology and sociology), on the other hand, may select his subject on the same grounds; but there the similarity usually ends. At some point in his data-gathering (and in the case of experimental research, prior to such experiments), he must "get organized," or prepare his research design. This process, while laden with that common sense which political scientists so admire, is rarely adhered to in international politics research. Simply put, it involves the following steps, though not necessarily in this sequence.

First, there must be an articulation of the question or questions to be answered, usually in the form of an hypothesis or hypotheses. This must describe, at the very least, a correlation in the incidence of certain specified phenomena, and at best, a causal relationship among such phenomena, and in each case should spell out the assumptions upon which the hypothesis is founded. Second, all of the variables (independent, dependent, and intervening) need to be defined as precisely and operationally as possible in order that they may be measured in terms of their frequency, direction, or intensity. (Parenthetically, such measurement by no means implies quantification, a belief which frequently repels those of us reared in the humanistic tradition. In many cases, it may only be necessary to be able to indicate that x is greater than y. On the other hand, the mere attempt to quantify, even if unsuccessful, may well produce a sharper, more clearly delineated set of variables.) Third, one must decide upon the sources of data to be employed and the techniques by which these data are to be gathered. If the source is written material, it may be examined by techniques ranging from systematic note-taking to highly schematized content analysis, and interviews may likewise be extremely open-ended or based upon a carefully preconceived interview schedule. During the data-gathering phase, the research design must provide for the avoidance of error in both reliability and validity, and only a carefully prepared design is likely to assure such avoidance. Finally, the research design needs to include the criteria which will be employed in interpreting the data collected and in reaching conclusions; the logical connection between these criteria and the prior articulation of assumptions and hypotheses should be obvious. To the extent that the data shed light on the questions posed, confirm or disconfirm the hypotheses, and produce a logically coherent set of conclusions clearly supported by the data, one has utilized an adequate research design.

Now much of this may seem to be a reiteration of the obvious, but the reader is challenged to name more than a small handful of articles, books, or monographs in the literature which actually articulate all relevant assumptions, state the hypotheses, and discuss the degree to which these latter have been borne out. In the absence of a research design providing for these characteristics, a piece of research may well produce some adequate description, but it is unlikely to be of much use for explanatory or predictive purposes. And the growth of theory is highly dependent upon explanation; causal relationships must remain the predominant concern of anyone purporting to contribute to such growth.

Specific behavioral science techniques

As to some of the more specific techniques peculiar to the behavioral sciences, it would be folly to attempt any full description or analysis of them here, but a few brief and

[12] Among some of the more valuable treatments of research design are: Jahoda, Deutsch, & Cook (1951, Ch. 3), Chapin (1947), Festinger and Katz (1953), Stouffer (1955), Goode & Hatt (1952), and Shils & Finch (1949).

introductory remarks may be in order. As suggested above, data collection often poses as much of a problem to the international relations scholar as does data interpretation, and the nonpolitical social sciences may hold some useful suggestions for us. Therefore, it might be appropriate to discuss two particularly useful methods of data gathering, one from existing sources, and one which requires that the data first be produced. The first of these is content analysis, which is perhaps the most important specific methodological contribution of the behaviorial sciences to those of us whose work is primarily with documents, memoirs, and histories.

Essentially, content analysis is nothing more than a highly systematic method of sorting and collecting information acquired from written (or spoken) communications. What is involved is the setting up of categories into which a preliminary examination of the material indicates most of it will fall. Such categories must, of course, stem from the conceptual framework within which the study is to proceed and should be able to produce answers to such questions as: Which arguments (and with what frequency) does the communication use in order to justify certain types of decisions? What sort of adjectives are employed how often to describe certain people, states, or other phenomena? What goals do the communicators attribute to their competitor? etc. The crucial thing is to devise categories which are mutually exclusive, unambiguous, and discernible, and this is best assured by a number of pretests conducted within a sample of the materials to be coded, with category modifications made as long as the researcher and his coders have any difficulty in deciding (*a*) what is and is not to be coded, and (*b*) into which category it must go. One may, in turn, code for broad themes, particular phraseology, explicit facts, or specific words, to mention just a few. Furthermore, one may code every sentence, entire articles, chapters, books, headlines, or whatever unit most suits the purpose of the study.[13]

[13] Among the outstanding discussions and applications of this technique are: Berelson (1952; 1954), Lazarsfeld & Barton (1951), and Pool (1959).

The charge is sometimes made that what is gained in precision and accuracy is lost in subtlety and insight—that by imposing a rigid categorization upon the material, one loses the essential flavor or fine qualitative distinctions. This may well be true, but it is usually a result of unimaginative category-building or the absence of a reasonably well-developed theoretical framework. Furthermore, the use of content analysis does not require that the written result be nothing more than a tabular or graphic report. Such results belong in an appendix or appropriately interspersed within a thoughtful, systematic, interpretive exposition. There is, however, a second criticism of the technique which does seem more justified, and that is the question of efficiency. Content analysis can be extremely time-consuming, normally requires several independent coders, and in the final analysis is still heavily reliant upon the intelligent inference. Though the charge is a legitimate one, it is answerable on two counts. First, one's methodology need be no more elegant than the purpose requires, and it seems unnecessary, for example, to adhere rigidly to the multiple and independent coding procedure in all cases; even if the lone researcher does his own coding, we have a more rigorous process than the traditional reading and intuitive note-taking. Second, the understanding of complex relationships cannot ordinarily be the result of serendipity or casual observation; it normally requires the output of considerable energy. If the discipline of international relations is to prosper, we will have to make such investments.[14]

Turning from the exploitation of existing data, let us examine briefly one of the techniques of the behavioral sciences which requires that the data be, in a sense, first produced and then interpreted. The general technique is known as survey research, and the interview is, of course, an integral and

[14] For some relevant applications of content analysis to this field, see the Stanford University RADIR Project studies, including Lasswell, Lerner, & Pool (1952a; 1952b). Truman (1952) wrote a well-balanced review of this series. Also see Berelson & de Grazia (1947), and several of the papers in Lasswell & Lietes (1949). Perhaps the most valuable combination of methodological discussion and application is George (1959).

familiar element. The purpose here is to ascertain the opinions, attitudes, beliefs, expectations, or recollections of a certain category of individuals. The sample, in turn, is drawn according to whose articulations the researcher is interested in; it might be a small number of government officials who are known to have participated in a particular decision or series of decisions, or it may be an entire nation, or perhaps even several nations. The larger the population involved, the more necessary it is to devise a sampling technique which produces a representative but manageable percentage of the population under study; this process itself is a highly complex one, with a multitude of methodological, statistical, and theoretical pitfalls.

Very briefly, the survey or interview technique requires the following processes. First (regardless of technique or methodology), one must pose the questions to be answered by the study; again, a formulation of hypotheses flowing from articulation of assumptions which are informed by some theoretical scheme is essential. Next, the researcher must decide upon the sort of information which will produce the attitudinal or other variables found in his hypotheses. From here, he begins to phrase the interview questions which will elicit these variables; normally the interview schedule (sequence of questions) will have to be revised several times, with a few pretests run to see whether it does in fact elicit the range of responses anticipated by the hypotheses. Simultaneously, the sample must be drawn, interviewers selected and trained, and the first round of results carefully evaluated for any flaws in the over-all design, the schedule, or the mode of interview. Once the data are in, the evaluative and interpretive process is similar to that employed in working from existing data; as a matter of fact, the results of the open-end or projective type of interview are often best utilized by the technique of content analysis.[15]

The virtue of survey research, from our point of view, is that it frees us of the age-old liability of having to say that American (or Indian, or French) public opinion was not ready for a certain policy, or that the more vocal segments of a given society believed that war was inevitable, and so forth, without ever really having an adequate basis for the statement. We all talk about public or elite opinion as an important variable in a state's foreign policy, but few of us have ever tried to identify and measure that opinion. As a result, what a certain group's opinion is or was is still very much merely a matter of opinion among the scholars concerned.[16]

Content analysis and survey research are but two of the many techniques developed and utilized so effectively by the behavioral sciences. As the student of international relations and comparative foreign policy becomes familiar with the literature outside of political science and history, he will discover—and may ultimately come to adapt and employ—a number of others of comparable value.

THE FINDINGS OF THE BEHAVIORAL SCIENCES

Turning from the methodological approach and techniques of these disciplines, we come to the second major area in which they may have something to offer the international relations specialist: their empirical findings. Reference here is to the empirical results of the vast amount of research undertaken in psychology, sociology, and anthropology since they became reasonably well-developed fields of inquiry. It may seem presumptuous of political scientists to assume that they can deal adequately with a subject deeply dependent upon personality, society, and culture with barely a glance at the work of those who have specialized in these areas, but again the fact is that most of us have almost totally ignored this vast body of literature. Worse than that, too many of us know little more about such findings than the layman, and as a result are frequently guilty of accepting and perpetuating the folklore and "con-

[15] Among the outstanding treatments of survey research are: Hyman (1956), Maccoby and Maccoby (1954), and Paul (1953).

[16] For some illustrative applications of these techniques, see Hero (1959), Buchanan & Cantril (1953), and Free (1959).

ventional wisdom" of our nonacademic contemporaries. Let us therefore refer (again, briefly) to a small sample of the sorts of behavioral science findings which might fill in some of the voids in our own literature, and perhaps correct or modify some of our more erroneous assumptions.

Turning to psychology first, several propositions which have emerged from long years of research suggest themselves to the interdisciplinary political scientist.[17] Perhaps one of the more significant conclusions to emerge from this discipline is that man is a multigoaled organism, pursuing at any one time a variety of goals, several of which may well be incompatible. Ignorance of this proposition has led many scholars to grief as they elaborated theoretical schemes postulating a unipurposeful human, preoccupied primarily (or exclusively) with physical survival, sexual gratification, political power, or material wealth. Nor have psychologists themselves been immune from such misleading determinism. But recent clinical, experimental, and social-psychological research—inductive and deductive—clearly confirms the multigoaled character of normal man. Perhaps the most persuasive evidence of this is the dominant contemporary tendency in psychology to speak of human motivation in terms of self-realization or maximization of gain; in either case provision is made for the highly useful phenomenological approach. This approach (and since it encompasses several divergent theoretical positions it can only be called an approach) presumes that there is likely to be some difference in the ways in which any two people perceive a situation, respond to alternatives, or pursue goals, depending upon a number of variables ranging from physical stature to cultural background. The maximization-of-gain concept permits us to deal with the fact that most men not only differ in their goal structures, but also rearrange their own goals, depending upon their perception of the situational field in which they find themselves at any moment. Such a finding may make social theorizing more difficult, but it at least has the virtue of minimizing the likelihood of oversimplifications based upon a nonexistent political, economic, or sexual man.

A second general finding of psychological research which is of considerable interest to the international relations specialist is that man is a highly malleable creature. The long-standing notion that belief and behavior are immutably molded by cultural milieu, educational environment, and emotional experience in one's youth has been thoroughly shattered by contemporary research in social psychology. Rather, it turns out that not only is there a wide variation in political and social beliefs within any given society and its subcultures, but that those beliefs which do exist at any given time are significantly susceptible to modification. Such susceptibility is, of course, a highly variable phenomenon, depending upon the subject area of the belief and the intensity with which it is held. But for our concern, it is worthwhile to note that most political beliefs are not held with very much intensity; and when they are, their objective referents are often so ill-defined as to permit rather ready manipulation. Again, it must be emphasized that not all beliefs are equally vulnerable to such modification. Values, for example, tend to be more rigidly held than the transitory attitudes which stem from these values, and the ephemeral opinions which derive in turn from both values and attitudes are more manipulable still.

Obviously, if opinions play a significant role in public and elite behavior, and if these are highly vulnerable to modification, one may reasonably anticipate that political behavior is likewise far from a constant or inflexible phenomenon. The result is that the student of international relations must continually consider the nature of the various actors' beliefs at the moment in which he is interested, and must likewise anticipate that the attitudinal predisposition of a large group of people may be quite different several days or weeks or months after its direction and intensity were last evaluated.

[17] Though these propositions are all conceivably susceptible to upset or invalidation, for the most part they have withstood numerous challenges successfully, and are today as well established as any propositions in the social sciences can ever be.

Such inconstancy is both a dependent and independent variable in relations among nations.

Leaving the field of psychology, we turn briefly to sociology in order to sample some of its findings. One of the more intriguing conclusions of sociological research is that the existence and awareness of an external threat does not necessarily lead to a greater degree of unity and a higher level of integration within the threatened society. Despite a widespread belief among political scientists to this effect, a number of studies seem to argue for a less certain conclusion. For example, if a society has already begun to experience disintegration and a deterioration in its social fabric, a sharp external threat might well hasten the disintegrative process.

Closely allied with this is the more tentative finding that two social groups in conflict (short of physical violence) may discover that the conflict itself may generate ultimate cohesiveness and union between them. Normally it is assumed that conflict drives groups (and nations) further apart, but if we appreciate that the absence of contact between groups can readily perpetuate mutual antipathy and hostility, we can begin to understand that even conflictful contact may initiate a process the end product of which is high compatibility or some form of organizational merger. The relevance of both of these propositions to international relations should be quite clear, especially to those of us concerned with political integration at the international level.

In the field of anthropology, it hardly seems necessary to point to any findings concerning our area of inquiry. Assuming that one of the sets of variables affecting the behavior of states is the cultural, the researcher need only turn to the work of those who have specialized in the region in which he is interested. Though not all the world's political units have been studied, and some of the research is singularly poor, one may still uncover an impressive amount of information concerning dominant values, elite selection, or domestic political patterns. Also of inestimable value are the Human Relations Area Files, in which eighty-eight major categories (each having from six to nine subcategories) of human activity are cross-classified with those regions which have been studied and reported upon by the anthropological profession.[18] And though many scholars may deride the Files, their utility to the uninitiated is considerable. It is also worth noting that the project has provided a stimulus to comparative work in cultural anthropology and should be equally useful in this regard to the student of comparative foreign policy.

THE CONCEPTS OF THE BEHAVIORAL SCIENCES

Let us turn now from the more obvious and mechanical contributions of the behavioral sciences to that which strikes this observer as perhaps the most crucial category of all—the heuristic. Though less clear and more indirect in their usefulness than either the techniques or the findings, over the long run the concepts of the behavioral sciences will probably emerge as the most relevant, especially in the development of theory. Rather than attempt to dwell at length upon the implications of one or two of the more promising concepts or notions, and still not do them justice, it might be more functional to mention a larger number and make brief reference to their possible relevance for international relations.

First, there are several conceptual schemes which have developed to such a degree that they might qualify under the rubric of "theories;" indeed, they are so labeled by many of those from whose disciplines they emerge. Most promising to this writer is that set of notions which we have come to call general systems theory.[19] Proceeding from the assumption that there are "structural similarities or isomorphies in different

[18] The Files are located and maintained (reasonably up-to-date) at the following universities: Chicago, Colorado, Cornell, Harvard, Hawaii, Indiana, Iowa, Michigan, North Carolina, Utah, Washington (Seattle), and Yale. Headquarters are at Yale, with a branch office in Washington, D. C. For a description of the Files, see Murdock et al. (1950). *A Guide to the Use of the Files* (1956) may be had free from Box 2054, Yale Station, New Haven, Connecticut.

[19] See especially von Bertalanffy (1956), Boulding (1956), and Miller (1955).

fields" and "correspondences in the principles which govern the behavior of entities that are intrinsically widely different," this approach seeks to identify such similarities and correspondences (as well as dissimilarities) as might be found in the universes of all the scientific disciplines. In its search for an integrative theory of behavior,[20] general systems theory postulates the existence of a system, its environment, and its subsystems. Some of the key concepts employed are "feedback," "homeostasis," "network," "entropy," and "information," reflecting a considerable intellectual debt to cybernetics. By thinking of the states as subsystems within the international system, which in turn has a particular environment of physical and social dimensions, we are provided with a rather fruitful taxonomy which suggests, in turn, a fascinating array of hypotheses. Within the same context, the idea of homeostasis is particularly suggestive to those of us concerned with balance, stability, and equilibrium in the international system.

Another set of concepts which seems to offer real promise is that employed in the theory of games.[21] The model is one of two or more players (individuals, groups, states, coalitions, etc.) pursuing a set of goals according to a variety of strategies. If the goals are perceived by the players as incompatible, in which only one may win, we have a so-called "zero-sum" game, with the players tending to utilize a "minimax" strategy; if they perceive a possible "win-win" outcome, their strategies tend to deviate sharply from the conservative minimax pattern in which they place prime emphasis on minimizing their own losses and maximizing those of the adversary. The appropriateness of such a model for bipolar international relations seems rather evident.[22]

Another range of possibilities appears when we consider the problem of inter-nation influence. From learning theory, stimulus-response theory, and the concepts associated with reinforcement, we might adapt and modify a wide range of models which could ultimately shed useful light on this central aspect of international relations. To illustrate: Is a major power more likely to shape the policies of a weaker neighbor by punishment, reward, denial, threat, promise, or calculated detachment? Or in seeking to explain the way in which public opinion in a given state ultimately influenced a certain policy decision, we might find some valuable suggestions in reference-group theory, the concepts of access and role-conflict, or some of the models of communication nets. To take another problem area, if one were concerned with the emerging attitudinal characteristics of the international environment, such notions as acculturation, internalization, relative deprivation, self-image, consensus, or animism might prove to be highly productive.[23]

The point which needs to be stressed here is that political science in general, and internation relations in particular, suffer from a serious paucity of concepts and models by which we might seek to describe, explain, or predict with fullness and clarity. Our conceptual poverty strikes this writer as perhaps the greatest single handicap in the development of everything from low-level empirical generalizations to a full-blown theoretical scheme. Some judicious and selective borrowing and adaptation might definitely be in order.

[20] Boulding (1956, p. 11) emphasizes that "it does not seek, of course, to establish a single, self-contained 'general theory of practically everything' which will replace all the special theories of particular disciplines. Such a theory would be almost without content, for we always pay for generality by sacrificing content, and all we can say about practically everything is almost nothing."

[21] On the theory of games, see the classic von Neumann & Morgenstern (1947); the excellent introductory collection by Shubik (1954); also Williams (1954) and Luce & Raiffa (1957). Of particular relevance for international relations are Schelling (1960) and Rapoport (1960).

[22] For a provocative treatment of the potentialities of "non-zero-sum" game theory, see Schelling (1958).

[23] Let it be emphasized, here as elsewhere, that I am by no means advocating a primitive sort of reductionism in which a model of interpersonal or intergroup relations is put through a translation machine and then applied lock, stock, and barrel to inter nation relations. I am merely suggesting that there is probably more of interest in such models than we have appreciated to date.

CONCLUSIONS

For some time, the author has awaited a general introductory statement on the role of the behavioral sciences in the study of international relations. Such a statement was deemed to be not only useful to his colleagues but essential to his graduate students during this promising period of ferment in the discipline. But none has appeared from the pen of more experienced and qualified scholars, and the perceived need remained. That a methodological and theoretical controversy has been brewing for some time cannot be denied, and the behavior of the opposing schools of thought has been far from satisfactory. The majority of us has dismissed the behavioral sciences with a cavalier disdain, while those more favorably disposed have seldom done more than utter agreeable platitudes about the importance of being scientific. Elsewhere (Singer, 1960), I have had the temerity to call for a temporary moratorium on "theorizing about theory," on the grounds that this discussion has been so ephemeral and vague as to be no longer intellectually fruitful. Perhaps it will not be too long before another can justifiably issue the same caveat regarding the role of the behavioral sciences in our field, but it is clearly not yet that time. On the other hand, no amount of this sort of academic speculation can serve as a substitute for research which actually attempts to utilize—with appropriate caution and discretion—the methods, findings, and models of these other disciplines.[24] And the primary purpose of this paper is to encourage those who might, by training or predisposition, be inclined to make that attempt.

REFERENCES

Apter, D. Theory and the study of politics. *Amer. pol. Sci. Rev.*, 1957, 51, 747–762.

Berelson, B. *Content analysis in communication research*. Glencoe: Free Press, 1952.

Berelson, B. Content analysis. In G. Lindzey (Ed.), *Handbook of social psychology*. Vol. 1. Cambridge: Addison-Wesley, 1954.

Berelson, B., & de Grazia, S. Detecting collaboration in propaganda. *Publ. Opin. quart.*, 1947, 11, 244–253.

Boulding, K. General system theory—the skeleton of science. In L. von Bertalanffy & A. Rapoport (Eds.), General systems. *Yearb. Soc. Adv. gen. syst. Theory*, 1956, 1, Part 1.

Buchanan, W., & Cantril, H. *How nations see each other*. Urbana: Univ. of Illinois Press, 1953.

Chapin, F. S. *Experimental designs in sociological research*. New York: Harper, 1947.

Deutsch, K. W. *Political community at the international level: problems of definition and measurement*. Garden City: Doubleday, 1954.

Deutsch, K. W. The place of behavioral sciences in graduate training in international relations. *Behav. Sci.*, 1958, 3, 278–284.

Deutsch, K. W., et al. *Political community and the North Atlantic area*. Princeton: Princeton Univ. Press, 1957.

Festinger, L., & Katz, D. (Eds.) *Research methods in the behavioral sciences*. New York: Dryden, 1953.

Fox, W. T. R. Interwar international relations research: the American experience. *World Pol.*, 1949, 2, 67–79.

Fox, W. T. R. (Ed.) *Theoretical aspects of international relations*. Notre Dame: Univ. of Notre Dame Press, 1959.

Free, L. *Six allies and a neutral*. Glencoe: Free Press, 1959.

Guetzkow, H. Isolation and collaboration: a partial theory of international relations. Pittsburgh: Carnegie Inst. Tech., 1956.

Guetzkow, H. Long range research in international relations. *Amer. Perspective*, 1958, 421–440.

George, A. L. *Propaganda analysis*. Evanston: Row, Peterson, 1959.

Goode, W. J., & Hatt, P. K. *Methods in social research*. New York: McGraw-Hill, 1952.

Harvard University. *The behavioral sciences at Harvard*. Cambridge: Harvard Univ. Press. 1954.

Hero, A. O. (Ed.) *Studies in citizen participation in international relations*. Boston: World Peace Foundation, 1959.

Hoffman, S. H. International relations: the long road to theory. *World Pol.*, 1959, 14, 355.

Hoffman, S. H. (Ed.) *Contemporary theory in international relations*. Englewood Cliffs: Prentice-Hall, 1960.

Hyman, H. *Survey design and analysis*. Glencoe: Free Press, 1956.

Jahoda, Marie, Deutsch, M., & Cook, S. *Research methods in social relations*. New York: Dryden, 1951, Vol. 1, Ch. 3.

Kaplan, M. *System and process in international politics*. New York: John Wiley, 1957, 172–173.

Lasswell, H., Lerner, D., & Pool, I. S. *Symbols of democracy*. Stanford: Stanford Univ. Press, 1952. (a)

Lasswell, H., Lerner, D., & Pool, I. S. *The com-*

[24] In a forthcoming volume this writer will attempt a more concrete and specific application of these disciplines to the study of international political relations.

parative study of symbols. Stanford: Stanford Univ. Press, 1952. (b)

Lasswell, H., & Leites, N. (Eds.) *Language of politics.* New York: Stewart, 1949.

Lazarsfeld, P., & Barton, A. H. Quantitative measurement in the social sciences. In D. Lerner & H. Lasswell (Eds.), *The policy sciences.* Stanford: Stanford Univ. Press, 1951.

Luce, R. D., & Raiffa, H. *Games and decisions.* New York: John Wiley, 1957.

Maccoby, Eleanor, & Maccoby, N. The interview: a tool of social science. In G. Lindzey (Ed.), *Handbook of social psychology,* Vol. 1. Cambridge: Addison-Wesley, 1954.

Miller, J. G. Toward a general theory for the behavioral sciences. *Amer. Psych.,* 1955, 10, 513–531.

Murdock, G. P. Outline of cultural materials. New Haven: Human Relations Area Files, Inc., 1950.

Paul, B. D. Interview techniques and field relationships. In A. L. Kroeber (Ed.), *Anthropology today.* Chicago: Univ. Chicago Press, 1953, 430–451.

Pool, I. S. (Ed.) *Trends in content analysis.* Urbana: Univ. Illinois Press, 1959.

Rapoport, A. *Fights, games, and debates.* Ann Arbor: Univ. Michigan Press, 1960.

Schelling, T. C. The strategy of conflict: prospectus for a reorientation of game theory. *J. Conflict Res.,* 1958, 2, 203–264.

Schelling, T. C. *The strategy of conflict.* Cambridge: Harvard Univ. Press, 1960.

Shils, E., & Finch, H. (Eds.) *The methodology of the social sciences.* Glencoe: Free Press, 1949.

Shubik, M. (Ed.) *Readings in game theory and political behavior.* Garden City: Doubleday, 1954.

Singer, J. D. International conflict: three levels of analysis. *World Pol.,* 1959, 12, 453–461.

Singer, J. D. Theorizing about theory in international politics. *J. Conflict Res.,* 1960, 4, 431–442.

Singer, J. D. The level of analysis problem in international relations. *World Pol.,* in press.

Singer, J. D. *State behavior in world politics.* Chicago: Rand-McNally, in press.

Sprout, Margaret, & Sprout, H. Man-milieu hypotheses in the context of international relations. Princeton: Center of International Studies, 1956.

Stouffer, S. Some observations on study design. *Amer. J. Soc.,* 1955, 50, 355–361.

Thompson, K. W. The study of international politics: a survey of trends and developments. *Rev. Pol.,* 1952, 14, 433–443.

Thompson, K. W. Toward a theory of international politics. *Amer. pol. Sci. Rev.,* 1955, 49, 733–746.

Thrall, R. M., et al. (Eds.) *Decision processes.* New York: John Wiley, 1954.

Truman, D. B. Book reviews. *Amer. Pol. sci. Rev.,* 1952, 46, 869–873.

Univ. of North Carolina. *Survey of behavioral science.* Chapel Hill: Univ. North Carolina Press, 1954.

Van Wagenen, R. W. *Research in the international organization field.* Princeton Center for Research in World Political Institutions, 1952.

von Bertalanffy, L. General system theory. In L. von Bertalanffy & A. Rapoport (Eds.), General systems. *Yearb. Soc. Adv. gen. syst. Theory,* 1956, 1, Part 1.

von Neumann, J., & Morgenstern, O. *Theory of games and economic behavior.* Princeton: Princeton Univ. Press, 1947.

Waltz, K. N. *Man, the state and war.* New York: Columbia Univ. Press, 1959.

Ware, Edith E. *The study of international relations in the United States.* New York: Carnegie Endowment, 1939.

Williams, J. D. *The compleat strategyst.* New York: McGraw-Hill, 1954.

Wisdom, J. *Philosophy and psychoanalysis.* Oxford: Blackwell, 1952.

Zimmern, A. *University teaching of international relations.* Paris: International Institute of Intellectual Co-operation, 1939.

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"Contrariwise," continued Tweedledee, "if it was so, it might be; and if it were so, it would be; but as it isn't, it ain't. That's logic."

LEWIS CARROLL, *Through the Looking-Glass*

In the preceding article ("The Relevance of the Behavioral Sciences to the Study of International Relations") Singer urged an interdisciplinary approach to the study of political science. In this paper he discusses Sidney Verba's *Small Groups and Political Behavior*, which suggests ways of "bridging the gap between the political and the scientific" and discusses the advantages and disadvantages of attempting to apply sociological and social-psychological materials and methods to the political process.

PRIMARY GROUPS AND THE POLITICAL SYSTEM: SIMULATION AND REALITY

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In increasing numbers, political scientists are beginning to appreciate the conceptual and empirical inadequacies of the field as traditionally defined, and as a result there is much casting about for help in other social science disciplines. But productive results of this search are by no means the inevitable reward for broad-mindedness and diligence. The problems of such borrowing are numerous, and while the intellectually provincial have tended to exaggerate them (particularly when balanced against the modest perceived benefits), the more cosmopolitan of us have tended to err in the opposite direction. More specifically, there has been a marked propensity to belittle or ignore the difficulties and dangers inherent in the application of models, data, and techniques from anthropological, psychological, and sociological concerns to those of an essentially political nature.

One of the many virtues of Sidney Verba's *Small Groups and Political Behavior* (1961) is that it commits neither of these sins. On the one hand, the book makes a serious effort to apply sociological and social-psychological materials to the political process, and thus refutes to some extent the justifiable allegation that most advocates of an interdisciplinary approach are only advocates, seldom practicing what they preach. On the other hand, Verba distinguishes himself by eschewing the temptation to evade or ignore the very real problems of such application.

Though the larger and most substantive part of the study summarizes and appraises the results of the standard small-group experiments regarding leadership, norms, and participation as major variables in group functioning, this review will make no effort to report or interpret these relatively familiar findings. Space limitations preclude any adequate treatment of the multitude of germane propositions which emerge, and these represent the less novel aspect of the book. Rather, I should like to focus on two of the more interesting issues that are raised, which bear directly upon the general problem of interdisciplinary research in political science. The first of these concerns the general relevance of primary groups to the political process in national or subnational systems, and their place in political and social theory.[1] The second, less familiar, concerns what Verba calls "bridging the gap" between experimental-group findings and the behavior of ongoing groups and organizations within the "real" system.

[1] For the sake of definition, Verba sees a primary group as one in which there is "face-to-face contact and communication" and mutual "awareness of each of the other members as separate and distinct entities" (p. 11). The implication is that its size may range from two up to about twenty, depending upon "the nature of the group, its task, the length of time it has existed, the membership, and so forth" (p. 12). Thus, one might include family, neighbors, classmates, peer groups, party cell, army team, bridge club, street gang, or subunit of larger, secondary groups.

THE RELEVANCE OF THE PRIMARY GROUP

In attempting to appraise the significance of small, face-to-face groups for the larger political system, one is led to the ever-present but seldom-analyzed level-of-analysis problem. No matter how completely we embrace a "system of action" approach (Parsons, 1937; Parsons et al., 1953) we must ultimately admit that our interest is not merely in "action" or "behavior," but in the action or behavior of social units or systems. These systems and their subsystems exist at a variety of levels, ranging from the individual to the world, and the level or levels we choose to focus upon will markedly influence our models and our methods.

Traditionally, the political scientist has opted for the larger or supersystemic level, and more particularly, the formal institutions of that level. Thus, most of our research and teaching has focused upon national, provincial, or local governments, and within these broad institutional focuses, we have concentrated our attention upon such specific institutions as legislatures, judiciaries, and executive or administrative agencies, or the constitutional relations between or among two or more such institutions.

Gradually, however, there developed some dissatisfaction with so formalistic and legalistic a focus, and increasing attention was devoted to political parties and electorates; with each, however, often perceived and dealt with as relatively homogenous institutions or populations. This trend eventually led to another step in the broadening process, culminating in greater concentration upon pressure groups and upon voting blocs, with the latter classified on the basis of race, religion, region, occupation, education, and other socioeconomic variables. And only recently has our emphasis expanded to the point where there is now a growing interest in the personality and opinions of individual citizens and (to a lesser degree) in the role of the smaller and less formal, face-to-face group. Thus, the range of alternative focuses for the study of politics (within nations) has steadily expanded so that one is relatively free to concentrate upon analytical levels whose size extends from the individual to the entire population of a nation, and whose degree of formal institutionalization extends from the family to the national government. Elsewhere, I have analyzed the implications of one's choice of level regarding *inter-nation* politics (Singer, 1961), bur to do so here regarding *intra-nation* politics would not be feasible. Suffice to say that such a choice does have serious theoretical (and methodological) implications for the researcher, and that each level of analysis (or combination thereof) offers a complex range of advantages and disadvantages, opportunities and obstacles, rewards and punishments. To date, political science has given little systematic and serious thought to these implications.

What justification does Verba see in the selection of the primary group as his level of analysis? He opens his defense by asserting that "to understand the processes of government, one must look beyond its formal structure." And after recognizing that the individual has received increasing attention in the discipline, he reminds us that "a significant level lies between that of the organization and of the individual personality: the level of the face-to-face group" (p. 17). Why is this level significant? He sees the primary, face-to-face group as performing two major sets of functions in the political system. One is the political socialization function, whereby the group plays an important role in shaping the attitudes, opinions, and subsequent political behavior of the individual member. After summarizing the accumulated evidence which demonstrates the degree to which small-group membership imposes a powerful pressure to conform, the author goes on to agree with Mannheim (1951, p. 181) that "whatever changes the New Age may bring, person-to-person relationships and primary groups will remain the basic character-forming agencies of society." For Verba, this socialization (or politicalization) process seems to take on a number of forms.

THE SOCIALIZATION PROCESS

First, the primary group (especially the family) tends to influence markedly the "political personality" of its products, and

consequently to shape the national character (or as some would have it, the "modal personality") of the population under consideration.[2] Second, experience in primary groups provides the skills necessary for participation in the political process. Third, it will influence the degree (political-apolitical) and nature (liberal-conservative) of one's subsequent participation. In addition to these relatively indirect effects, primary-group socialization is seen as having the more direct result of strongly affecting such specific political acts as voting and active party membership, mainly because of the intermediary role which primary-group leaders play in communicating and interpreting the mass media content to their members.

Having demonstrated to his own satisfaction the "influence of the small group on the political attitudes and behavior of its members" (p. 45),[3] the author moves on to the more concrete relationship of such groups to the system as a whole. Here the concern is one of further specifying the nature of the primary group's impact on the system. The first point is that this impact differs significantly from society to society (or system to system), particularly as the society is either traditional or modern. For example, there is the contrast between Japan and the United States, wherein the former system tends to put primary-group, particularistic loyalties above those of "the impersonal political system" as a whole (p. 47).

Next, Verba addresses himself to political stability and by indirection to the ubiquitous question of whether conflict and stability are functional or disfunctional.[4] Recognizing the degree to which small groups are in conflict with the larger system and with the intermediate secondary groups, and the range of destabilizing functions the "deviant" ones play, he is nevertheless undisturbed. First of all, the alarmist view that "face-to-face influence is inconsistent with political choices made on a rational democratic basis" is seen as reflecting "an artificial, rational model" (p. 51). Moreover, he prefers to emphasize the ways (besides socialization) in which these groups *do* enhance the stability of the system: providing (*a*) a source of affective ties which mass society can seldom provide, and (*b*) an "element of flexibility into the operation of the formal system" (p. 59).

Though it clearly makes the case for relevance, this part of *Small Groups and Political Behavior* leaves one with a less-than-satisfied feeling. That is, one is persuaded that they *are* important as independent variables, and one is told the variety of ways in which they affect the system, but perhaps in too fragmentized a fashion. It is unfortunate that Verba fails to attempt any serious synthesis of his propositions into a more formal theoretical presentation. On the other hand, this may be demanding too much. Obviously, a reviewer will always look for additional, or different, emphases than those selected by the author, and it is essential that we respect the freedom of the latter to focus upon that which he considers most relevant and to give less attention to that which concerns him less. Moreover, one can hardly suggest that social and political theory is in such an advanced state that it is merely a matter of "plugging in" to an already existent formal structure. I would hope, however, that he or others will soon be tempted to follow up this work by developing, adapting, and testing such models as may shed light on the precise function and role of primary groups in political systems. Until this is done, our confidence in the importance of

[2] Verba avoids the continuing controversy among social scientists regarding national character, and settles for passing references to Mead, Gorer, Rodnick, and Schaffner, and the useful critique by Leites (1948). Some additional evaluations of the concept and its application are Nett (1958) and Farber (1955).

[3] He is careful, however, not to imply any "simple primary group monism," and reminds us that "political behavior is not determined solely by the predispositions that an individual brings into the political process from his experiences and training in primary groups. It is also affected by the way in which the [larger] political system interacts with these predispositions" (p. 37).

[4] But oddly enough, we find no allusion to Georg Simmel (1904) or his contemporary interpreter, Coser (1956). Two of the more sophisticated discussions of this problem in its political context are Easton (1953) and Truman (1951).

the primary group will continue to rest upon a relatively intuitive basis, and the theoretical link between the individual and the system will remain vague and ill-defined.

CONTROLLED EXPERIMENTS AND THE POLITICAL WORLD

The second set of problems raised in this study concerns the applicability of small-group experimental research to the ongoing political world, and it is here that *Small Groups and Political Behavior* makes an impressive contribution to political and social theory and method. As opposed to the prevalent tendency of behaviorally-oriented political scientists to *postulate* the applicability of experimental research to the system itself, he provides us with a thoughtful and provocative analysis of the methodological and conceptual virtues and difficulties which such application involves.

What are the advantages offered, and what difficulties are generated in their pursuit? In his chapter "Culture of the Laboratory", the author gives the political scientist a coherent and systematic summary of the opportunities and obstacles confronting the researcher who would "create his own miniature political process in the laboratory" (p. 61). The dominant virtue is seen to be "the control that the experimenter has over the situation. By manipulating certain carefully selected variables and by controlling [holding constant], ideally, all others, he can achieve clear and unambiguous connections between the independent variables he controls and the dependent variables he is studying" (pp. 63–64). Of course, these "carefully selected variables" are a function of one's theoretical model, and the weaker this model is, the closer one comes to "barefoot empiricism."

To be more specific, what are the sorts of controls Verba postulates as important, and how difficult is it to achieve them? Moreover, what sorts of concessions to artificiality does he consider as the price of precision? First, he discusses the problem of population sample, noting the discrepancy between the serious concern of the survey researcher (Campbell et al., 1960) and that of the small-group experimenter. Obviously we may generalize with far greater confidence from a carefully-drawn sample of an entire population than we can from a haphazard (or more likely, a skewed) sample representing a very special segment of any population; i.e., volunteers self-selected from among a student body, or captive subjects who happen to be enrolled in a psychology course. Not only is this a sample which is culture-bound and subculture-bound, but one whose members are painfully aware that they *are* subjects.

This self-consciousness problem leads, in turn, to consideration of "observer effect." The experimenter or observer may attempt to minimize his impact on the subjects by standardizing or concealing his role; in the latter case, one may resort to any technique from the "stooge" to the one-way mirror. But there are very few situations into which subjects can be brought for experimental purposes while remaining oblivious to its artificial or contrived nature. In addition, the experimental subjects tend to have no more than what Verba calls "their special and limited commitment to the experiment" (p. 87). This means that the generation of affect, tension, high motivation, etc., will be less likely; that the solution of internal structure problems will have to accompany task fulfillment; and that considerations of a postexperimental nature will have minimum influence. Finally, the experimental task assigned may well be so novel that the subjects will dismiss it as inconsequential or unreal, and thus exhibit behavior extremely remote from that likely to occur in an ongoing and actual situation. For all of these reasons, there must be a profound gap between "the special culture of the laboratory and the real world of social behavior."

As to bridging this gap, our author first reminds us that it may not be as wide as it appears, and that political situations are as many and varied as laboratory ones are artificial or misleading. A telling point, too, is his observation that such a "real-world" act as voting may often have much less emotional commitment than participation in a small-group experiment. Another similarity adduced is that between an experi-

mental group assigned an administrative decision-making task and the many governmental interagency committees which are set up on an *ad hoc*, short-lived basis, with most of the members often meeting each other for the first time. And, as Verba reminds us, how affective *are* the relations among bureaucrats?

Having noted the nature of this nonetheless real chasm, he addresses himself next to the matter of making the experiment as "life-like" as possible. One technique is to seek to simulate political reality in the experimental situation, and here he refers us to such mock systems as the Chicago and St. Louis jury studies (Strodtbeck et al., 1957), or the RAND air-defense center, replete with all the equipment and visual inputs which characterize the real thing. Under this rubric, he discusses the use of identical personnel (using real jury panels or Air Force personnel as subjects), highly similar information inputs (recordings of court cases or simulated radar scope displays), and suspension of disbelief (by emphasizing how the experiment may influence the real world).

Rather than attempt to "create" artificial groups, another approach is to "move the laboratory into the field," and he reports on a number of relevant studies which attempted exactly this. Whether this is still a "laboratory experiment" in the literal sense is open to question, and will be discussed below; the author himself observes that "the line between the natural field experiment and the laboratory experiment is not a sharp one" (p. 94).

Verba then goes on to tackle the methodological problems of applying small-group findings to larger, secondary groups: organizations, parties, bureaucracies, pressure groups, and governments themselves. Urging that "if the experimental method is to supply useful results for understanding [larger] organizations, it will have to be through the transfer of results in experimental small groups to the level of organizational behavior," he nevertheless recognizes that "this transfer is fraught with difficulties" (p. 100). Using the small-group simulation of inter-nation relations (Bloomfield & Padelford, 1959; Guetzkow, 1959) as an example, he suggests that such simulation "may still develop and test a number of useful hypotheses." Moreover, because it includes the element of human variability, this sort of simulation is said to offer certain advantages over verbal and mathematical simulations.

As to the transferability of small group propositions to their larger setting, Lewin (1951, p. 164) is quoted as believing that this could be done via "transposition, . . . a change which leaves the essential structural characteristics unaltered." Verba himself sees "certain identities in form and structure" between, for example, primary groups and organizations. Referring to small-group work by Bales (1950), Borgatta and Bales (1953), and Carter (1950) and the organization theory of Barnard (1938) and Simon (Simon, 1957; March & Simon, 1958), he notes a substantial degree of interlevel convergence in terms of role, structure, and function. But he is careful not to go too far, reminding us that this "does not mean that these systems are identical; the relationship between the two levels is a relationship between analogous, not homologous, mechanisms" (p. 108).

This part of the study concludes with an excellent restatement of the research strategy involved in bridging the gap. First, the primary-group studies must not remain in the "isolation of the laboratory," but rather the hypotheses evolved there must be tested in the field, and vice versa. Second, field studies, surveys, and historical studies must continue to play a major role, with experimental studies merely a link in the larger chain of research.

CONCLUSIONS

Though he is careful to qualify his embrace of the small-group experiment, the general tone is one of considerable enthusiasm, and it might be worthwhile to conclude by articulating a number of further questions. First, a few minor quarrels. At one point, it is suggested that laboratory or other simulation of the real world can not only develop, but "*test* a number of useful hypotheses" (p. 100). If by "test" the author

refers only to some appreciable increment in the credibility of the hypothesis, there is no dispute, but if the word is meant to imply confirmation or disconfirmation, then I must sharply disagree. He would probably be on safer ground if he restricted his justification of this or any other simulation to the suggestive and the heuristic.

At another point, Verba finds the experimental model preferable to the verbal or mathematical one because it does not "leave out the element of human variability" (p. 101). It is not at all evident that by using humans rather than verbal or mathematical symbols we achieve a higher degree of isomorphism with the real world, especially as in this case, the world of real nations. My guess would be that any modern computer, programmed by (or at the direction of) a competent international relations specialist, would produce "behavior" more akin to that of a nation-state than would any individual student or small group of students. The virtue of the "nonprogrammed" students, however, is that they may generate a range of potentially promising hypotheses that might otherwise never have occurred to even the most imaginative researcher.

This observation leads, in turn, to my other serious criticism of the book (the first being that it is too incomplete an attempt to place the primary group into a well-formulated theory of politics). This is the occasional tendency, albeit often qualified, to discount the impressive possibilities which "natural" experiments contain. It seems that Verba has capitulated too readily to the professional folklore, which has it that political phenomena are too complex, elusive, and discrete to permit of systematic, scientific analysis. Thus, he would have us move from the "uncontrollable bustle and flux of the political process" to the limited and manipulable environment of the laboratory, where we can approach a precision and replicability otherwise unattainable. Is this absolutely essential? Is it not possible to select a number of comparable historical cases, develop indices or criteria for the relevant variables, and then test our hypotheses by observing (or even measuring) the correlations which emerge? Now no one would contend that the correlations emerging from such "ex post facto experiments" provide us with causal propositions. But, on the other hand, may causality be legitimately inferred from those correlations observed in the more controlled experimental setting? There would seem to be as much room for error in the social science laboratory as there is likelihood of oversight in post hoc analyses of "real" cases.

That the historian (or more frequently, the historiographer) and some political scientists may not be too wide of the mark in this regard is suggested by Ernest Nagel (1961, p. 457). After noting (a) that "the bulk of controlled empirical inquiry in the social sciences is not experimental in the sense we have associated with this term," and (b) that the variables cannot be planned or manipulated at will, he nevertheless observes that "the investigations satisfy to a greater or lesser degree the requirements for controlled empirical inquiry." I would be the first to admit that most neo-experimental efforts have met with only limited success, but would explain this on the grounds that the attempt has been made too infrequently, and often by those whose methodological innocence was, though less than most, still too great. My prediction would be that in the near future we will be analyzing actual political cases, after the fact, as if in a reconstructed experiment. An increasing volume of data is becoming available, and our models and methods are improving rapidly.

In conclusion, this is one of the most important studies to come along in our discipline in many years. It is creatively designed, thoughtfully arranged, thoroughly researched, and carefully written. By attacking some of the most crucial questions in the theory and method of political studies, and dealing with them in a rigorous and imaginative fashion, Professor Verba has made an important contribution to bridging the gap between the political and the scientific in political science.

REFERENCES

Bales, R. *Interaction process analysis.* Cambridge: Harvard Univ. Press, 1950.

Barnard, C. *The functions of the executive.* Cambridge: Harvard Univ. Press, 1938.

Bloomfield, L., & Padelford, N. Three experiments in political gaming. *Amer. pol. Sci. Rev.*, 1959, 53, 1105–1115.

Borgatta, E., & Bales, R. Interaction of individuals in reconstituted groups. *Sociometry*, 1953, 16, 302–320.

Campbell, A., *et al. The American voter.* New York: John Wiley, 1960.

Carter, L., *et al.* A further investigation of the criteria of leadership. *J. abnorm. soc. Psychol.*, 1950, 45, 350–358.

Coser, L. *The functions of social conflict.* Glencoe: Free Press, 1956.

Easton, D. *The political system: an inquiry into the state of political science.* New York: Alfred Knopf, 1953.

Farber, M. (Ed.), New directions in the study of national character. *J. soc. Issues.*, 1955, 11, 3–57.

Guetzkow, H. The use of simulation in the study of inter-nation relations. *Behav. Sci.*, 1959, 4, 183–191.

Hare, A., *et al.* (Eds.), *Small groups: studies in social interaction.* New York: Alfred Knopf, 1955.

Leites, N. Psycho-cultural hypotheses about political acts. *World Pol.*, 1948, 1, 102–119.

Lewin, K. *Field theory in social science.* New York: Harper, 1951.

Mannheim, K. *Freedom, power, and democratic planning.* London: Routledge and Kegan Paul, 1951.

March, J., & Simon, H. *Organizations.* New York: John Wiley, 1958.

Nagel, E. *The structure of science: problems in the logic of scientific explanation.* New York: Harcourt, Brace & World, 1961.

Nett, Emily. An evaluation of the national character concept in sociological theory. *Soc. Forces*, 1958, 36, 297–303.

Parsons, T. *The structure of social action.* New York: McGraw-Hill, 1937.

Parsons, T., *et al. Working papers in the theory of action.* Glencoe: Free Press, 1953.

Simmel, G. The sociology of conflict. *Amer. J. Sociol.*, 1904, 9, 490–525, 672–689, 798–811.

Simon, H. *Administrative behavior.* New York: Macmillan, 1957.

Singer, J. D. The level of analysis problem in international relations. *World Pol.*, in press.

Strodtbeck, F., *et al.* Social status in jury deliberation. *Amer. sociol. Rev.*, 1957, 22, 713–719.

Truman, D. *The governmental process.* New York: Alfred Knopf, 1951.

Verba, S. *Small groups and political behavior: a study of leadership.* Princeton: Princeton Univ. Press, 1961.

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When I say that a thing is true, I mean that I cannot help believing it. I am stating an experience as to which there is no choice. But as there are many things that I cannot help doing that the universe can, I do not venture to assume that my inabilities in the way of thought are inabilities of the universe. I therefore define the truth as the system of my limitations, and leave absolute truth for those who are better equipped.

OLIVER WENDELL HOLMES, *Collected Legal Papers*

BOOK REVIEWS

A. Hoffer and Humphery Osmond. *The chemical basis of clinical psychiatry.* Springfield, Illinois: Charles C. Thomas, 1960.

In this book, aimed primarily at the clinician, Drs. Hoffer and Osmond offer their famous adrenochrome hypothesis in a fairly complete and revised form. Beginning with a well-written chapter stating the authors' orientation towards research and scientific methodology, the book proceeds through a brief historical recounting of biological studies in schizophrenia and psychotomimetics to explore the points of conflict between the biological and functional approaches to psychiatry.

The book really begins at Chapter 4, titled "Relationship of Mood to Catechol Amines," in which the authors present the hypothesis that the level of anxiety depends upon the ratio of epinephrine to a dihydroxyindole metabolite, more or less identified with 5,6-dihydroxy-N-methylindole, which is thought to be derived from epinephrine via adrenochrome. In this scheme "epinephrine has the role of maintaining drive, interest, and emotion. Dihydroxyindole has the role of protecting against the anxiety caused by epinephrine" (p. 64). Adrenochrome also allays anxiety (p. 41), as does adrenolutin (p. 63). Both adrenochrome and adrenolutin, especially the latter, are also associated with undesirable psychological changes.

From this point on this reader began to feel as though he were touring Alice's Wonderland on a pogo stick. This odd sensation seemed to be due to the compounding of hurried writing and incomplete arguments, resulting in half quotes, misquotes, and important omissions of the relevant literature, contradictions in arguments, and inconsistencies in the very data presented. Indeed, the book seems not a book in the usual sense but rather a hurried compilation of the authors' papers, and its aim seems more to persuade than to demonstrate.

As an example of the unanswered problems in this book, if one juxtaposes the equation between anxiety and the epinephrine to 5,6-dihydroxy-N-methylindole ratio with the adrenochrome hypothesis in Chapter 13, which states, in essence, that schizophrenics do not adequately convert epinephrine to dihydroxy-N-methylindole but instead show an increased conversion of adrenochrome to adrenolutin (p. 211), one is immediately faced with the problem of explaining an anxious schizophrenic. A number of workers have demonstrated that there is no essential difference in blood epinephrine levels (and thereby no difference in the anxiety factor) between schizophrenic and nonschizophrenic subjects, and both adrenochrome and adrenolutin are anxiety-lowering; therefore, increased production of adrenolutin might produce psychological aberrations but not, by the authors' scheme, anxiety. And yet anxious schizophrenics do exist, and not uncommonly. Furthermore, injection of epinephrine in male schizophrenic and nonschizophrenic subjects leads to behavioral or verbally expressed anxiety in about 60% of subjects of either diagnostic group, but neither overt nor expressed anxiety in the remainder. Would not one also expect from the adrenochrome hypothesis an exacerbation of psychotic symptomatology as epinephrine is converted to still more adrenochrome and adrenolutin? Yet several authors have reported this not to be the case.

And from where does this anxiety stem? Epinephrine passes the blood brain barrier only poorly if at all, but even granting that brain epinephrine levels parallel blood levels, where are we led? The authors cite the work of Marrazzi, who found that epinephrine inhibits synaptic conduction in the trans-callosal system, and the work of Leimdorfer and Metzner, who found that interventricular epinephrine produces sleep and surgical anesthesia, but the relationship between these findings and "anxiety production" is never made clear.

Besides this sort of incomplete elaboration, some of the internal inconsistencies do de-

serve mention as a forewarning for readers. In discussing a proposed mechanism for lysergic acid diethylamide (LSD) action the authors state on page 135, for example, that LSD increases the activity of the adrenal medulla but that "adrenocortical activity is not altered which suggests that this is specific for LSD. . . ." On page 138, however, they quote evidence that "LSD acts by potentiating epinephrine to increase adrenocorticotropic hormone (ACTH) activity," On the following page the authors make the astonishing statement that "LSD acts as its own best antagonist by depleting the brain of epinephrine," a statement which finds its parallel in a case history on page 137 in which a normal "subject was given penicillimine and then LSD with the result that the subject had no feeling of emotion for the two weeks that followed the experiment." The authors interpret this by saying "the penicillimine-LSD combination seems to have depleted brain epinephrine stores so greatly that it took two weeks for them to be replenished." Nowhere is it made clear that there is no evidence for an LSD mediated release of brain catecholamines and that this inference is apparently drawn, quite unjustifiably, from release of adrenal epinephrine via the pituitary adrenal axis. The authors do state, in the text, that the mechanism of LSD action is unknown; but this statement applies to all pharmacological agents and does not seem, to this reviewer, a sufficient disclaimer to avoid leaving the reader with the impression that nonetheless evidence does exist for LSD release of central stores of catecholamines.

Misquotes of the literature that are encountered in this book are exemplified by the statement on page 78 that "serotonin levels are increased in the brain by the following four methodological substances: (1) tranquilizers, e.g., reserpine; (2) anticonvulsants; (3) central nervous system depressants, e.g., phenobarbital; (4) central nervous system stimulants, e.g., amphetamine; (5) 5-hydroxytryptophan (5-HTP)." In point of fact, of course, total brain serotonin levels are decreased by reserpine, increased by the anticonvulsant diphenylhydantoin, decreased in dog brain but unchanged in cat brain by amphetamine, and increased by 5-hydroxytryptophan. Indeed, the effects of amphetamine and reserpine are stated by the authors in the preceding paragraph!

The half review of the literature is also a frequent occurrence in this volume. While pointing out that reserpine releases both catecholamines and serotonin from the brain, the authors fail to mention that methyl-18-o(3-dimethylaminobenzyl) reserpate releases up to half of brain catecholamines without either significantly releasing serotonin or eliciting sedation. At higher concentrations both are released and sedation is induced.

The authors quote one paper demonstrating that norepinephrine can combine with pyridoxal phosphate and thereby inhibit 5-HTP decarboxylase. They fail to mention the earlier demonstration that Dopa and Dopamine (the precursors to norepinephrine and epinephrine) inhibit Dopa decarboxylase, nor do they mention the evidence indicating that Dopa decarboxylase and 5-HTP decarboxylase are the same enzyme. They quote Wooley's report of nicotinamide sedation but not the relationship between serotonin and nicotinamide cited in the same paper.

Finally, there are a number of important omissions. The very pertinent paper of Szara and Axelrod is not even mentioned, much less discussed, although the title of one section, "Is Adrenochrome Present in the Body," is remarkably akin to the title of their paper. Similarly, the authors briefly review a number of bioassay procedures and assert that ample evidence exists that schizophrenic plasma contains substances not found as a rule in patients who do not suffer from the illness, without discussing the very serious objections and contrary findings in the literature.

This brief set of examples, which could be expanded considerably, indicates that readers of this book should exercise caution in evaluating its central thesis and prepare themselves beforehand with a background of the literature. The formulated hypotheses in this book are both brilliant and ingenious. Their hypothesis may prove correct, but supporting evidence is not convincing. Clinical psychiatry is much in need of inclusive and testable hypotheses, and it is far better to advance an erroneous theory than to accept

a state of chaos. But as Sherrington suggested, "One should entertain theories, but not be entertained by them."

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Otto Von Mering. *A grammar of human values.* University of Pittsburgh Press, 1960.

A Grammar of Human Values is the twenty-first in a series of monographs written as part of the Harvard Values Project. Its author attempts several tasks. These are (1) the development of a general theory of valuation, (2) a test of a hypothesis on the effects of culture contact on valuing, (3) a typology of styles of valuing or realms of valuing, and (4) the use of a fairly new instrument in a field situation. Philosophy, sociology, anthropology, and social psychology are drawn upon. From the point of view of social psychology this book presents some interesting virtues (mainly as a result of its breadth) and some severe defects (poor organization and obscure writing).

Starting with a brief review of the work in values, especially that of Durkheim, Parsons, Mannheim, Kluckhohn, and Merton, the author proceeds after some digression to develop his theory of valuation. Valuation is seen as a process in which a "trending" is established towards "definitive" or "probable" values, from among the range of "possible" values. This occurs by "weighting," the result being commitment in action. Although heavily footnoted, the theory is highly nonspecific, and lumps values, attitudes, beliefs, and motives together indiscriminately.

Somewhat unrelated to the theory is the central hypothesis ". . . as breadth but especially as intensity of personal contact with peoples from other cultures increases individual and group standards of valuation tend to become less simple in content and more comprehensive."

The site of the empirical investigation was the by now much-studied southwestern communities of Rimrock and Homestead. Individuals in the communities, and the communities as a whole, had differing degrees of contact with nearby Navahoes. The measure of intercultural contact (IC) was obtained from questionnaire responses to a social distance scale towards the Navaho, and a weighted ratio of the number of Navahoes that the respondent had to dinner to the number that he had as friends. Among other problems this ratio can produce the anomaly of the man who knows one Navaho and has him over for dinner getting a higher IC score than the man who knows four Navahoes and has two over for dinner. The "grammar of values" consists of four types of values or styles of valuing. These types seem to be produced by the combining of two dichotomies: sympathy vs. lack of sympathy, and wide applicability vs. narrow. These types (and many subtypes under each) are used in coding discussions among 3-person groups on the subject of their relations with the Navaho. A moderator is present to keep the group on the subject, and to get people started by initially presenting them with a stimulus conversation. This is called the Controlled Discussion Technique, and its use constitutes one of the more uncommon and interesting features of the study.

If we forget for the moment the fact that, at best, the IC score measures attitude towards the Navaho (social distance scale combined with a rather meaningless measure of contact), the results could be taken as being in strong support of something like the hypothesis.

In discussing the results the author tends to get far beyond his data and talk about the characteristics of the different "cultures" when the data is obtained only from a sample of men, and only from their discussions on a particular subject, albeit an important one in the southwest. At the same time, much of his discussion of individual value profiles is highly suggestive and could lead to some interesting work. In particular, it raises the problem of the causes and consequences of deviance that "moves with" the mode, as well as that which "moves away" from it.

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LETTERS TO THE EDITORS

April 19, 1961

Editors, *Behavioral Science*

In my opinion the authors of the very valuable paper "The Factor Structure of Variables Concerned with Coronary Artery Disease" (January, 1961) failed to point out an important inference from the factor analytic data which they reported in the article. The superiority of the factor analytic approach in their study lies in the very fact that not one but two independent dimensions of coronary failure were extracted (ignoring a third dimension with a loading of only .21 on the criterion). One dimension emphasizes serum total cholesterol and serum lipid phosphorus, with an additional but smaller emphasis on systolic and diastolic blood pressure. The second dimension stresses the body characteristics of height and weight (negatively), and history of coronary failure in siblings. It would appear from these findings that the authors are dealing not with one but with two separate conditions associated with coronary artery disease. Whereas the first condition is associated with an inhibitory personality pattern of emotional expression and with some cyclothymia, the second condition is characterized with the reverse, an expressive pattern of emotionality and noncyclothymic (even) temperament.

If the above inference is plausible, as it seems to be, then the use of the discriminant function across all predictors simultaneously is not advisable since any prediction thus obtained fails to differentiate between the two separate conditions. By combining the two conditions, the predictors may cancel their discriminating power since their prediction may be in opposite directions in the two conditions. This is particularly true in view of the fact that the resulting lambda weights are relative to each other. Prediction would be considerably improved if weights were obtained for each respective dimension by means of two separate multiple regressions.

SALOMON RETTIG, *Columbus Psychiatric Institute and Hospital, Columbus, Ohio*

May 29, 1961

Editors, *Behavioral Science*

Salomon Rettig courteously afforded me the opportunity of reviewing his comments. Clearly there are two or more factors associated with coronary disease. These factors are necessarily fewer in number than the variables associated with coronary disease. These data are useful in deciding whether to be "lumpers" or "splitters" in regard to variables concerned with coronary disease. Among other considerations the decision will depend upon the relative frequencies of the factors. In part these frequencies are measured by the per cent of total variance attributed to each factor. Consider, for example, the unusual type of "coronary" (factor) regularly associated with the presence of a rare but critical variable such as xanthomatosis (gout, renal disease, or diabetes) which contributes a very low per cent of variance. Xanthomatosis is rare, but the conditional probability of coronary disease developing in a patient with this diagnosis is high.

The inference that there are two conditions (factors), each with some components which may cancel a disease effect of the other factor, seems plausible to me. It's reasonable to think that prediction by separate regressions for each dimension will give greater accuracy than one regression.

Throughout the United States a number of major investigations are underway to study and analyze data simultaneously on most of the variables concerned with cardiovascular diseases. The psychological, dietary, and physical activity aspects of coronary disease are the least tractable to reliable measurement and analysis. Factor analysis and regression methods are particularly appropriate for studies of the epidemiology of heart disease.

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COMPUTERS IN BEHAVIORAL SCIENCE

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Editorial Note: Computers in Behavioral Science does not as a rule publish detailed descriptions of computer programs, but limits itself to abstracts of such descriptions. An exception was made in this case in order to underline and encourage the shift to symbolic languages in computer programming.

A General Cross-Classification Program for Digital Computers,

Roland R. Bonato and Bruce D. Waxman, Center for Health Statistics, Department of Health, Education, and Welfare and the National Institutes of Health.[1]

INTRODUCTION

One of the distinguishing features of contemporary research in the social sciences is the large amounts of data that are utilized. This condition arises primarily from a need to consider numerous characteristics of large populations. In most instances investigators devote considerable time to the ordering of data into frequency distributions prior to formal statistical analysis. This ordering of data generally takes the form of tabular presentation where a dependent variable or count is simultaneously cross-classified by several independent variables. Data processing of this sort is usually done with punch-card equipment where the variables to be considered are first punched on cards, then sorted into some order, and finally run through a tabulating machine. These procedures will accomplish the task of cross-classification with varying degrees of ease and expense, depending on (1) the number of cases to be processed, (2) the number of variables to be cross-classified simultaneously, (3) the number of separate cross-classifications required, (4) the input and output format required, and (5) the number of separate punch cards containing information for a given individual.

The advent of digital computers with abilities to store large quantities of data and to perform calculations at speeds measured in microseconds made possible convenient solutions to these data-processing problems.

The program described below illustrates one of several possible applications of computers to cross-classification.

GENERAL PROGRAMMING

Recourse to computers has not always been feasible because professional programmers are not available and the investigator cannot afford the time necessary to learn programming. Also, the task of writing and testing a new program for each cross-classification problem may discourage computer utilization. Consequently, the development of general programs promises to facilitate use of the computer for tabulating procedures. As used here, a general program is one which has wide application but does not require special knowledge of programming or the operation of the program itself. Ideally, a general program would produce cross-tabulation tables from raw data according to parameters specified by the user.

Although the program presented here was developed for a specific research project, tabulation of the data was sufficiently diverse to encourage the composition of a general procedure.[2] The program prescribes a method

[1] This computer program has been developed by the authors in co-operation with Dr. James Sakoda of the Department of Psychology, University of Connecticut. Acknowledgement is made to the Computer Center at the Massachusetts Institute of Technology where the program was developed on an IBM 704. Both authors were formerly with the State Department of Health in Hartford, Connecticut.

[2] This program was developed in conjunction with the research activities of the Family Injury

where parameters, such as the number of cases, are punched into cards and presented to the computer as data rather than being internally contained in the program itself. Thus, a different set of parameters or controlling variables are used to modify the program at the time of data input. Utilization of the program mainly entails learning the meaning and proper presentation of the control variables.

To facilitate discussion, the controls are grouped or classified according to (1) input, (2) computing, and (3) output procedures.

Prior to input, the program assumes that the data have been edited for errors. Input to the computer may be from cards or tape, and data from several successive cards may be combined to form a single record; i.e., information from a maximum of 20 cards may be selected and treated as a single case. Thus, the equivalent of a "long" punch card is created by the computer with the total number of columns equal to the sum of the columns containing information. The format for this "long" card is determined for each set of data at the time of input by a format control card. The format control card is read in to the computer immediately prior to the data and specifies the number of cards as well as the number of columns occupied by pertinent and nonpertinent information. Thus, one or two format control cards allow the program to handle most data card layouts.

Regrouping or recoding of data from its original form to intervals of specified size is also possible by means of control cards.

Computation of cross-classification tables requires several control cards specifying parameters, such as number of cases, and the number of variables (sex, age, education, etc.). For any given table, a maximum of four variables may be cross-tabulated. If a five-fold table is desired, e.g., variables $A \times B \times C \times D \times E$, the program cannot be used unless the data is sorted and processed separately.

When cross-tabulation includes more than 20 variables or when a different set of data is to be processed, new control cards must be read in to the computer. The number of different sets of data and/or sets of 20 variables are referred to in terms of cycles. For example, if data A must be cross-tabulated for 40 variables and data B for 12 variables, the computer will perform three cycles to complete the task.

The output format is fixed, i.e., internally contained in the program and not subject to control by the user at the time of input. Each output table is assigned a numerical identification, but it is also possible to read in alphabetical identification.

In summary, the program may be applied to most relatively straightforward cross-classification procedures by means of control cards. Where complex cross-tabulations are not required, the program permits the investigator to assume personal control over an often tedious clerical task with a minimum investment of time and effort.

CONTROL CARDS FOR THE GENERAL CROSS-CLASSIFICATION PROGRAM

The discussion which follows provides the working instructions necessary to use the IBM 704 general program developed to produce two-, three-, and four-fold cross-classifications with optional coding and table identification.[3] Preparation of the control cards discussed below is necessary to make the program operative.

General description

1. The program will process N number of cases and M number of variables where $N = 1$ to 9,999 and $M = 1$ to 20 in a single cycle.
2. A maximum of 999 cycles using 20 variables at a time is possible.
3. Cross-classification may be made in terms of two-, three-, or four-fold tables.
4. A new format statement for data input may be read into the machine for each cycle.
5. Output tables are limited to size 20

Survey of the Connecticut State Department of Health and is supported by a research grant from the National Institute of Mental Health.

[3] A modification of this program has been developed and tested for the IBM 709 by Richard Enburg. Copies of this modification can be obtained by writing to the Connecticut State Department of Health.

by 100; i.e., the first variable cannot contain more than 20 categories, the second not more than 100. The limits on the size of the third and fourth dimensions are 999.

6. Coding by addition, subtraction, division, or special code is optional.
7. Data may be read from cards or tape.
8. Identification of output tables is optional.
9. Tape 4 is used for input, tape 2 for output, and 5 and 6 for intermediate storage of results. Tapes 2, 5, and 6 must always be present.
10. The number of 2-, 3-, and 4-fold tables that can be processed in a single cycle is limited to 72; i.e., 72 combinations of 20 variables.

Input procedure

All control cards must be read in on-line. The first card defines the number of cycles needed. The second and third cards to be read are for the purposes of identification and are limited to 72 alpha-numeric characters per card. The fourth card defines the following control information:

1. NTOT, the total number of cases to be cross-classified.
2. N must be set equal to 1,250, if the number of cases exceeds 1,250. If N is less than 1,250, N = NTOT.
3. NOFV, the number of variables to be used in one cycle. The NOFV cannot exceed 20, in any one cycle.
4. NVAR, the number of 2-, 3-, or 4-fold tables to be run per cycle (72 or less per cycle).
5. IDENT. If greater than 0, table identification cards must be present. If 0, cards are not required.
6. M24. If the number of tables (NVAR) is 1–24, 25–48, or 49–72, M24 will equal 24, 48, or 72.
7. NCODE, a signal to the 704 that the recoding subroutine will be used. If NCODE is greater than 0, recoding will take place. If NCODE equals 0, the recoding subroutine will be bypassed.
8. NKD, number of variables to be coded (set equal to 20).
9. NSAME rewinds input tape 4, if greater than 0. This is used when the same data are required for more than one cycle.
10. NPR causes data to be read from cards if greater than 0, or from tape 4 if 0 or less.

The fifth through eighth cards give the numerical "name" of the variables required for cross-tabulation tables. For example, the first three columns of the fifth card identify the first variable used for table 1; the next three columns signal the location of the first variable needed for table 2; and so on to table 72 if 72 tables are required. (It would, in fact, take three "fifth" cards to signal the "names" for a full 72 variables, since the "name" of each variable occupies a three-column field.) The sixth card indicates the location of the second variable needed for cross-tabulation. The seventh and eighth cards signal the location of the third and fourth dimension of the tables. If a 2-dimensional table is required, the appropriate columns of the seventh and eighth cards are left blank. If a 3-dimensional table is needed, the proper columns of the eighth card are left blank.

The ninth card contains the highest coded number for each variable, i.e., if a variable contains categories such as 1 = male and 2 = female, then 2 is the highest code number. The number of categories must appear in ascending variable number order, i.e., the 704 will determine the number of categories for the lowest numbered variable from the first three columns of the ninth cards, for the second lowest variable from the next three columns, etc. (If a particular variable is recoded using the coding subroutine, the number of categories should equal the number produced by recoding.)

The tenth and eleventh cards contain the format controls for reading data. Construction of format control cards requires the following symbols: a left and a right parenthesis enclosing the format specifications, nX to specify that n columns of data will be skipped, mIn to specify that the following m number of variables, each occupying n columns, are to be read into the 704, a comma separating each specification, and a

slash (/) signifying "go to the next card." For example, the symbolic expression (12*X*, 2*I*2/3*X*,*I*3) will cause the computer to skip columns 1–12 of the first card, read columns 13 and 14 as the first variable, read columns 15 and 16 as the second variable, "go to the next card," skip the first three columns, and read columns 4, 5, and 6 as the third variable. If the format specifications exceed card capacity (72 columns), the remaining format may be punched into a second card. Although a complete format control may be contained on one card, a second blank card must be present.

If recoding is required, additional cards must be punched. The first card contains information on the kind of recoding necessary. Once the recoding routine is entered, all the variables are considered in ascending order. If a particular variable does not require coding, the proper columns are left blank. A 1 indicates coding by addition, a 2 by subtraction, a 3 by division, and a minus 1 calls for a special code. The next card contains the magnitude of the constant to be added, subtracted, etc., to the variable. The special code allows data to be grouped in several ways. To use the special code, a MBRAK card specifying the upper limit of each category is required and one card must be present for each variable that is to be recoded. For example, if age is to be coded into five categories, i.e., 0–4, 5–14, 15–44, 45–64, 65 plus, the MBRAK card should read: --4-14-44-64100 (assuming data go no higher than 100).

SUMMARY

Card	*Description*	*Format*[4]
1	NPROB = number of complete cycles required to get all tables	(I3)
2	Identification	(72 alpha-numeric)
3	Identification	(72 alpha-numeric)
4	NTOT, N, NOFV, NVAR, IDENT, M24, NCODE, NKD, NSAME, NPR	(2I4, 8I2)
5–8	NVOR = numeric "name" of variables to be cross-tabulated	(24I3)
9	MHI = number of categories for each variable	(24I3)
10	Format for data	(72 alpha-numeric)
11	Format for data	(72 alpha-numeric)
12	KIND = kind of coding, i.e., addition, subtraction, division, and special code	(24I3)
13	NUMB = the number by which adding, etc., is carried out	(24I3)
If special code is used, then:		
14	MBRAK = upper limit of each recoded category	(24I3)

In addition to the above cards, one card (72 alpha-numeric characters) may be read in to identify each table of output. If cross-classification is by two variables only (2-dimensional), only one card of identification is necessary. If, however, cross-tabulation is by three or four variables, one card of identification must be present for each subtable; i.e., if the third dimension is a 3-category variable and the fourth dimension is a 2-category variable, then six cards of identification are necessary.

OUTPUT

The table number and the variable "names" involved in each cross-tabulation appear first. This is followed by the table size. If the table is a 3-way or 4-way classification, each subtable will be identified by a "dimension" count; i.e., if the third variable contains two categories, the first subtable will read "third dimension = 1" and the second subtable will read "third dimension = 2." If table identification cards were specified, the identification contained on these cards will immediately precede the

[4] It is crucial that the format specifications of each variable control card be observed. Unless each piece of control information is located in its proper field on the control card, improper results will be produced. The number of columns on each card to be used for each constant are determined by the expression *mIn*, where *m* is the number of constants per card and *n* the number of columns devoted to each constant. Cards 2 and 3 can contain either alphabetic or numeric information. This information can be placed anywhere on these cards. Cards 10 and 11 can contain only those symbols used in specifying format controls for reading data. These symbols, however, need not be placed in any specific columns. The remaining cards can contain numerics only. Information punched to the right of column 72 is not read by the 704.

cross-tabulation matrix. The first column and last row in each table contain the totals. The lower left corner cell contains the complete table total.

LIMITATIONS

Cross-classification by a 0 category will not occur. Wherever 0's appear, that piece of information is lost. If the 0 code was used in punching data, this may be corrected by using the coding subroutine (add plus 1 to each category). The minus or 11 punch, the plus or 12 punch, a punched 0, and a blank are all interpreted as 0 by the 704. Consequently, if data have been punched using the 11 and 12 punch, the cards must be revised.

An additional restriction is encountered if more than 1,250 cases are to be processed at one time. This restriction is caused by the length of the tape (standard tapes are 2,400 feet long). The value of

$$\sum_{i=1}^{NVAR} (LIMIT_{1i} \times LIMIT_{2i} + 33) \times LIMIT_{3i} \times LIMIT_{4i}$$

(where $LIMIT_{ji}$ refers to the number of categories for variable j of table i) must be less than 900,000. If the sum exceeds this limit, there will not be space available on the intermediate storage tapes for the complete intermediate results. To overcome this lack of storage the program should be run two or more times, each time producing only as many tables as the above restriction allows.

If a variable is to be recoded and more than 1,250 cases are to be processed, a complete set of MBRAK cards, following the binary program and the control cards, must be present for each group of 1,250 cases, or partial group. Thus if 2,500 cases were to be processed two sets of MBRAK cards would be required, while if 2,501 cases were to be processed three sets of MBRAK cards would be required.

EFFICIENCY

The speed with which the 704 will solve a specific cross-classification problem is dependent on the time consumed in each of its several phases, i.e., input, computing, and output. Approximately 5,000 cases (20 variables per case) can be tape-read to the 704 in 15 minutes. Normally, four minutes are consumed by the machine in the writing of output. However, cross-classification of variables containing numerous categories increases output time. Therefore, it is difficult to estimate output time accurately since this will vary with each problem. As a rule of thumb, the computer time can be roughly estimated by assuming that 15 minutes is required for each 5,000 cases of input and that an additional five to ten minutes are required for computing and writing of output.

A loss of efficiency is produced by general programming since certain arbitrary conditions have to be created to achieve maximum generality. This can be illustrated by reference to the DIMENSION statement in the FORTRAN listing which follows. The term LAD (20, 1,250) reserves storage for a maximum of 1,250 cases, each associated with 20 variables. Thus a total of 25,000 memory units are reserved for data. In addition, the term NTAB (20, 100) reserves storage for a matrix (table) having a maximum of 2,000 cells. As a result of these conditions, 1,250 cases are read in, cross-tabulated, and stored on intermediate tapes so that the next 1,250 cases can be processed and added to the intermediate tapes. Obviously, greater efficiency could be achieved if more cases were read in at one time. To do this, however, it would be necessary to consider fewer variables and/or allow for a smaller output matrix since the maximum storage of the 704 is 32,760 binary words. Thus efficiency is gained at the expense of generality. For those investigators who cannot afford to program their research problems, it is probably cheaper to have one relatively inefficient program which can handle most problems than continually to write new programs for each new job.

SYMBOLIC PROGRAMMING

The FORTRAN program which follows provides an example of symbolic programming, i.e., a set of instructions that are ultimately but not directly utilized by a computer. Symbolic languages, such as

FORTRAN (Formula Translation), must be translated by the computer into a "machine language."

Whereas the programmer is most familiar with decimal-mathematical symbols, most computers respond only to binary machine symbols. Since programming in machine language has many disadvantages, most modern computers are equipped with a translator which converts programmer-oriented instructions to computer-oriented language.

The FORTRAN translator is a series of machine language instructions which cause the 704 to convert FORTRAN statements into binary instructions. The translator also identifies and informs the programmer of errors in the logic and the punctuation of the FORTRAN program. Unlike symbolic languages such as SAP, there is no one-to-one correspondence between FORTRAN and machine instructions. A single FORTRAN statement produces several machine language instructions. Furthermore, the translator automatically assigns memory locations for the program, the data, and the output.

The original FORTRAN (FORTRAN I) consists of 32 types of statements including input and output statements, arithmetic statements, and dimension and transfer statements. These types of statements are illustrated in the program given here. Each line represents a FORTRAN listing as it actually appears on an IBM card.

The second, third, and fourth lines of the program constitute a dimension statement in which all subscripted variables and the number of memory locations associated with each variable must appear. Note that the word DIMENSION, the commas, and the parentheses are FORTRAN conventions, whereas LAD, MVAR, etc., are variable names composed by the programmer. If the word DIMENSION is misspelled or a comma is misplaced, the translator will signal an error and a binary program cannot be produced.

The input statement READ 7, NPROB means "read the value of NPROB into a preassigned memory location from a card (as opposed to tape) according to the format specified in statement 7."

An example of a transfer statement appears immediately before 495 (sequential numbering of statements is not required). IF (IDENT) 495, 480, 480 signifies "if the memory cell containing the magnitude of IDENT is negative, execute 495 as the next instruction; if zero or positive, execute statement 480." A transfer of this kind will determine whether the recoding routine will be executed or bypassed.

Finally, the arithmetic statement NDONE = NDONE + 1 causes the computer to increase the value of NDONE by one. This single FORTRAN statement is translated by the computer into the following set of machine operations: "first, clear the accumulator unit (where addition takes place) of its previous contents; then, bring in the value of NDONE from memory to the accumulator; next, add the contents of a memory cell containing the constant one; and finally, store the new magnitude in the NDONE memory cell."

Further explanation of certain FORTRAN statements are provided in the cross-classification program.

THE FORTRAN PROGRAM

Since it is impossible for the authors to distribute reproduced binary decks of the program, the FORTRAN source program is listed below. Once this program has been punched on cards, any IBM 704 installation can translate it into machine language. If the program itself is to be modified, the listing below may help in the development of different routines.

Listing[5]	*Annotation*
C R.BONATO N 752-180 CROSS CLASSIFICATION PROGRAM	

[5] It has been the authors' experience that 704 installations differ somewhat in their procedures. It is possible that at some installations certain of the FORTRAN statements listed may be unnecessary or illegal. If such is the case, the difficulty should be resolved by reference to the diagnostic that the 704 will produce.

Listing	*Annotation*
`DIMENSION LAD (20,1250),MVAR (72,5),MHI (24),NTAB (20,100),NTAPE 1(20),FMT (24),NRTOT (100),NCLT (20),KIND (20),NUMB (20),MBRAK (20) 2, NVOR ( 72,4)`	
`NDONE = 0`	NDONE is not a subscripted variable so a dimension is not required
`DO 499 J = 1,20`	Do everything from this statement to 499, 20 times
`DO 499 I = 1,100`	For each value of J do 499, 100 times
`499 NTAB (J,I) = 0`	
`C Read in Controls`	
`READ 7,NPROB`	
`500 READ 5,NTOT,N,NOFV,NVAR,IDENT,M24,NCODE, NKD,NSAME,NPR`	Read controls from cards according to format no. 5
`5 FORMAT ( 72H R.BONATO N 752 – 180 X CLASS 1 / 72H 2 / 2I4,8I2 )`	
`WRITE OUTPUT TAPE 2,5`	Write on tape the above identifying information
`READ 7,(( NVOR (J,I), J = 1,M24 ), I = 1,4 )`	Read the "names" of the variables to be cross-classified; 4 cards
`READ 7, ( MHI (J), J = 1,24)`	Read one card containing the highest number of categories for each variable
`7 FORMAT ( 24I3 )`	
`DO 4 I = 1,4`	
`4 WRITE OUTPUT TAPE 2,8,I, ( NVOR (J,I), J = 1,NVAR)`	Write on tape the variable "names" originally read in
`8 FORMAT ( 14H DIMENSION NO. I3, (I6,33I3 ))`	
`WRITE OUTPUT TAPE 2,9, ( MHI(J), J = 1,NOFV )`	
`9 FORMAT (//27H HIGHEST NO.CATEG.EACH VAR. //(24I3 ))`	How to write out above statement: 27 alphabetics and 24 fields, 3 columns wide
`READ 10,(FMT (J), = 1,24)`	Read in format control cards
`10 FORMAT ( 12A6 )`	Format for the format control card
`REWIND 5`	Move intermediate Tape 5 to its beginning
`REWIND 6`	
`NSW = 3`	Place a 3 in the memory cell assigned by the computer to NSW
`NCOUNT = 0`	
`NDONE = NDONE + 1`	
`IF ( IDENT ) 495,480,480`	
`495 DO 490 I = 1,NOFV`	Do everything from 495 to 490 x number of times and x is the value of memory cell assigned to NOFV
`READ 82`	
`490 WRITE OUTPUT TAPE 2,82`	
`C FIND HIGHEST AND LOWEST VARS. NEEDED FOR TABLES`	
`480 LOW = NVOR (1,1 )`	Low is set equal to the magnitude of location (1,1) of the NVOR matrix previously read in
`LHI = NVOR (1,1 )`	
`DO 161 J = 1,NVAR`	

Listing	*Annotation*
DO 161 I = 1,4	
IF (NVOR (J,I)) 161,161,162	
162 IF (LOW − NVOR (J,I))163,163,164	
164 LOW = NVOR (J,I)	
163 IF (LHI − NVOR (J,I)) 165,161,161	
165 LHI = NVOR (J,I)	
161 CONTINUE	A dummy statement signifying "no operation"
C FIND KEY VARIABLES AND CHANGE WORK CODE IN MVAR	
JOAN = 1	
DO 170 KC = LOW,LHI	
DO 167 JK = 1,NVAR	
DO 167 IK = 1,4	
IF (KC − NVOR (JK,IK)) 167,166,167	
166 MVAR (JK,IK) = JOAN	
LO = NVOR(JK,IK)	
167 CONTINUE	
IF (KC − LO) 170,169,170	
169 JOAN = JOAN + 1	
170 CONTINUE	
DO 472 JX = 1,NVAR	
IF (NVOR(JX,3)) 468,468,469	
468 MVAR(JX,5) = 2	
GO TO 472	An unconditional transfer to 472 if operations arrive at this statement
469 IF (NVOR(JX,4)) 470,470,471	
470 MVAR(JX,5) = 3	
GO TO 472	
471 MVAR(JX,5) = 4	
472 CONTINUE	
12 DO 11 KLEAR = 1,NOFV	
DO 11 KLAR = 1,N	
11 LAD (KLEAR,KLAR) = 0	Clear the input storage matrix of its previous contents
IF (NPR) 14,14,16	
14 READ INPUT TAPE 4,FMT,((LAD (J,I),J = 1,NOFV), I = 1,N)	Read in data from tape according to the format control card
GO TO 18	
16 READ FMT, ((LAD (J,I),J = 1,NOFV), I = 1,N)	Read data from cards
18 NCOUNT = NCOUNT + N	
IF (NCODE) 19,19,20	If NCODE is negative or zero bypass the recode statements
C SUBROUTINE CODE 0 = NO CODE NECESSARY,	
C 1 = ADD,2 = SUBTRACT,3 = DIVISION, −1 =SPECIAL I.E. HIGHEST NO. OF CATEGORIES	
20 IF (N − NCOUNT) 483,485,485	
485 READ 7, (KIND (J), J = 1,NKD)	Read recode control card KIND
READ 7, (NUMB (J), J = 1,NKD)	Read recode control card NUMB
483 DO 32 J = 1,NKD	
NOPER = KIND (J)	
IF (NOPER) 21,32,22	
21 READ 7, (MBRAK (JJ) , JJ = 1,20)	Read control card specifying special recoding
NOPER = 4	
22 DO 29 I = 1,N	
GO TO (23,24,25,26) ,NOPER	

Listing	*Annotation*
23 LAD (J,I) = LAD (J,I) + NUMB (J)	Recode by addition
GO TO 29	
24 LAD (J,I) = LAD (J,I) − NUMB (J)	Recode by subtraction
GO TO 29	
25 LAD (J,I) = LAD (J,I) / NUMB (J)	Recode by division
GO TO 29	
26 JU = 1	
34 IF (LAD (J,I) − MBRAK (JU)) 27,27,28	Special recoding
27 LAD (J,I) = JU	
GO TO 29	
28 JU = JU + 1	
GO TO 34	
29 CONTINUE	
32 CONTINUE	
C START LOOP FOR ALL TABLES FROM 1,NOFV	
N = 1250 MAX CASES	
19 DO 115 K = 1,NVAR	
LOCV = MVAR (K,1)	Locate the first variable to be cross-tabulated
LOCV2 = MVAR (K,2)	Locate the second variable
JBIG = MHI (LOCV)	
IBIG = MHI (LOCV2)	
KBIG = 1	
LBIG = 1	
IF (MVAR (K,5) −3) 33,31,30	
30 LOCV4 = MVAR (K,4)	If operations arrive here a 4-fold table is required
LBIG = MHI (LOCV4)	
31 LOCV3 = MVAR (K,3)	If operations arrive here, a 3-fold table is required
KBIG = MHI (LOCV3)	
C LOOP FOR 2-3-4 CROSS.TAB.,ONE SET OF VAR.AT A TIME	
33 DO 115 L = 1,LBIG	
DO 115 J = 1,KBIG	
DO 45 I = 1,N	
C 2-3 WAY BRANCH	
IF (KBIG − 1) 40,40,35	
35 IF (LAD (LOCV3,I) − J) 45,37,45	
37 IF (LBIG − 1) 39,40,39	
39 IF (LAD (LOCV4,I) − L)45,40,45	
40 L1 = LAD (LOCV,I)	
L2 = LAD (LOCV2,I)	
NTAB (L1,L2) = NTAB (L1,L2) + 1	Tabulation in the proper cell of the output table occurs
45 CONTINUE	
IF (NTOT − NCOUNT) 75,75,47	If negative or zero then data are exhausted so table is completed
C BRANCH FOR EITHER TAPE 5 OR TAPE 6	
47 GO TO (50,60,60) , NSW	
50 DO 57 II = 1,IBIG	
READ TAPE 5, (NTAPE (LL), LL = 1,JBIG)	
DO 55 JJ = 1,JBIG	
55 NTAPE (JJ) = NTAPE (JJ) + NTAB (JJ,II)	Prior intermediate results are added to new intermediate results
WRITE TAPE 6, (NTAB (KK),KK = 1,JBIG)	
56 DO 57 KLER = 1,JBIG	
NTAPE (KLER) = 0	
57 NTAB (KLER,II) = 0	
GO TO 115	

Listing *Annotation*

```
C     STORE FROM TAPE 6 TO 5
   60 DO 70 III = 1,IBIG
      IF ( NSW - 2 ) 62,62,65
   62 READ TAPE 6, ( NTAPE (LLL), LLL = 1,JBIG )
   65 DO 67 JJJ = 1,JBIG
   67 NTAPE (JJJ) = NTAPE (JJJ) + NTAB (JJJ,III)
      WRITE TAPE 5, ( NTAPE (KKK),KKK = 1,JBIG)
   69 DO 70 KLEE = 1,JBIG
      NTAPE (KLEE) = 0
   70 NTAB (KLEE,III) = 0
      GO TO 115
C     START OF OUTPUT                          Row, column totals and tabulations are written on Output Tape 2
   75 IF (MVAR (K,5) - 3 ) 76,78,79
   76 WRITE OUTPUT TAPE 2,80,K,NVOR (K,1),NVOR
        (K,2),JBIG,IBIG
   80 FORMAT (//10H TABLE NO.I5,17H     VARIABLE NO.
     1  I5,9H VRS.VAR.I5 // 13H TABLE SIZE = I5,4H BY
        I5 //)
      GO TO 86
   78 WRITE OUTPUT TAPE 2,77,K,NVOR (K,1),NVOR
        (K,2),NVOR(K,3),JBIG,IBIG
     1, J
   77 FORMAT (//10H TABLE NO.I5,17H     VARIABLE NO.
        I5,9H VRS.VAR.I5,5H
     1VRS.I5 // 13H TABLE SIZE = I5,4H BY I5 // 16H
        3RD DIMENSION = I5
     2 // )
      GO TO 86
   79 WRITE OUTPUT TAPE 2,72,K,NVOR (K,1),NVOR
        (K,2),NVOR (K,3),NVOR (K,
     14),JBIG,IBIG,J,L
   72 FORMAT (//10H TABLE NO.I5,17H     VARIABLE NO.
        I5,9H VRS.VAR.I5,5H
     1VRS.I5,5H VRS.I5 // 13H TABLE SIZE = I5,4H BY
        I5 // 16H 3RD DIMEN
     2SION = I5,16H 4TH DIMENSION = I5 //)
   86 IF ( IDENT ) 84,84,88
   88 READ 82
   82 FORMAT ( 72H
     1                    )
      WRITE OUTPUT TAPE 2,82
   84 NGRTO = 0
      DO 100 IAI = 1,IBIG
      IF ( NSW - 2) 85,87,90
   85 READ TAPE 5, ( NTAPE (LAL), LAL = 1,JBIG )
      GO TO 90
   87 READ TAPE 6, ( NTAPE (LBL), LBL = 1,JBIG )
   90 DO 98 JAJ = 1,JBIG
      IF ( NSW - 2 )95,95,92
   92 NTAPE (JAJ) = 0
   95 NTAB (JAJ,IAI) = NTAB (JAJ,IAI) + NTAPE (JAJ)
      NRTOT (IAI) = NRTOT (IAI) + NTAB (JAJ,IAI)
   98 NCLT (JAJ) = NCLT (JAJ) + NTAB (JAJ,IAI)
      NGRTO = NGRTO + NRTOT (IAI)
      WRITE OUTPUT TAPE 2,105,NRTOT (IAI),( NTAB
        (LY,IAI),LY = 1,JBIG )
  105 FORMAT ( I8,3H    2015 )
      NRTOT (IAI) = 0
      DO 100 KLR = 1,JBIG
      NTAB (KLR,IAI) = 0
```

Listing *Annotation*

```
  100 NTAPE (KLR) = 0
      WRITE OUTPUT TAPE 2,106,NGRTO,( NCLT (KAK),
        KAK = 1,JBIG )
  106 FORMAT ( // 2I8,19I5)
      DO 112 JEAN = 1,JBIG
  112 NCLT (JEAN) = 0
  115 CONTINUE
C     END OF OUTPUT, START OF TAPE SWITCH
  118 IF ( NTOT - NCOUNT - 1250) 135,120,120
  120 N = 1250
      GO TO 135
  125 IF ( NTOT - NCOUNT ) 150,150,130
  130 N = NTOT - NCOUNT
  135 IF ( NSW - 2 ) 145,140,140
  140 NSW = 1
      REWIND 5
      REWIND 6
      GO TO 12
  145 NSW = 2
      REWIND 6
      REWIND 5
      GO TO 12
  150 CONTINUE
      IF (NPROB - NDONE ) 160,160,155
  155 IF( NSAME ) 500,500,157
  157 REWIND 4
      GO TO 500
  160 CONTINUE
      END FILE 2
      STOP 77777
      END ( 0,1,0,1,1 )
```

(Manuscript received January 4, 1961)

The Rote Marketer[1], *George H. Haines, Jr., Carnegie Institute of Technology.*

The purpose of this paper is to discover the decision processes used by members of a specific task-oriented group. The task is that of producing the monthly marketing decisions required for the Carnegie Tech Business Game, described by Cohen, Cyert, Dill, Kuehn, Miller, Van Wormer, and Winters (1960).

The specific theory presented below is consistent with the general concepts of Newell, Shaw, and Simon (1958). In particular, it is postulated that there exist: (1) a memory, (2) search and selection procedures, and (3) a set of rules or criteria which guide the decision-making process (Clarkson, 1960). One way of specifying precisely these three elements is by use of a computer program. This is the device adopted in this study. For a more complete discussion of the issues involved in the use of this tool, see Newell and Simon (1959).

Usually heuristic computer programs can modify their behavior by eliminating unsuccessful procedures and adapting to changing conditions in the "environment." This is true of the model presented here also. The present study postulates that the basic organization of such decision-making procedures is epicyclic in nature.

The "upper level" of this performance structure is the part which performs the over-all planning function, e.g., which learns in the sense indicated above. The output of this program takes the form of a set of policy parameters. The policy parameters control the general behavior of the "lower level" program.

The "lower level," which is the part of

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the process studied in greatest detail in this paper, accepts as inputs the policy parameters set by the "upper level" program plus the necessary data from the world. It then proceeds to make decisions in a set, mechanical way. Although this program responds to the environment, the decision structure it uses is fixed. For this reason it can be termed a rote program, using rote to imply a set, mechanical way of making decisions. This general type of decision-making process is not inconsistent with certain propositions in organization theory, as will be noted below.

By definition, all goals are influential in decisions. One particular goal of the teams studied seemed to be of particular relevance, however. The teams tended to set two operational goals. The first was continued existence. Such a goal and its implications have been discussed for actual firms by Fellner (1958). The second goal of the teams (if they perceived no danger of going out of business) was doing as well as the other teams in the industry. This goal is characterized by statements such as: "B-O is our chief competitor, so let's set all prices next period equal to their prices this period." The essence of such a mechanism is a simple response to the environment. For example, policy may be set so as to make the nature of the simple response move in the same direction as everyone else.

It is easy to note in the ensuing discussion that in such rote performance programs very few features of the "complex environment" are used in making the decisions. It is not sufficient to leave the analysis at this point. Above the rote performance program lies a fairly sophisticated analysis—a program with the ability to calculate consequences and deal with uncertainty by imagining alternative responses to the environment. This more complicated planning program appears to be only infrequently invoked, however.

The actual performance of people in reaching decisions was studied in four cases. In one case shorthand notes of the decision process were taken; in another some formal, taped protocols were obtained; in two cases no formal notes of the decision process were taken.

As protocols of the upper level of decision are not easy to obtain, it was decided to study first the rote program. Through this a clearer understanding of how and where the planning function enters into the total process of decision-making could be gained. Once this is known, the rare chance of observing a planning decision could be made more fruitful by having an understanding of what the outputs of the planning program were. Thus this report will deal primarily with the rote performance programs.

PERFORMANCE PROGRAM STRUCTURE

The performance programs fall into clearly defined areas. These may be listed:

1. Advertising Decisions
2. Price Decisions
3. Case Sales Forecasting
4. Distribution Expenditure Decision

For each of these areas a detailed program was developed. The programs were written in GATE, a computer language for the IBM 650 with disc storage. Copies of the programs can be obtained from the author.

Advertising decision program

This program produces advertising decisions for the next month. It is an example of the response mechanism type of decision-making discussed previously. A flow sheet is shown in Figure 1.

In the process outlined in Figure 1, the program responds to the environment differently according to certain cues. For purposes of exposition, let us assume that all control variables are greater than zero. Then we can discuss the qualitative behavior of the program. First, it tests whether "share of market" is increasing. If "share of market" has fallen, the response mechanism is more likely to increase advertising expenditures than if "share of market" has risen.

After this, the program tests the firm's advertising expenditures for the past month against its competitors. Usually, the sensitivity of this test is controlled by a variable

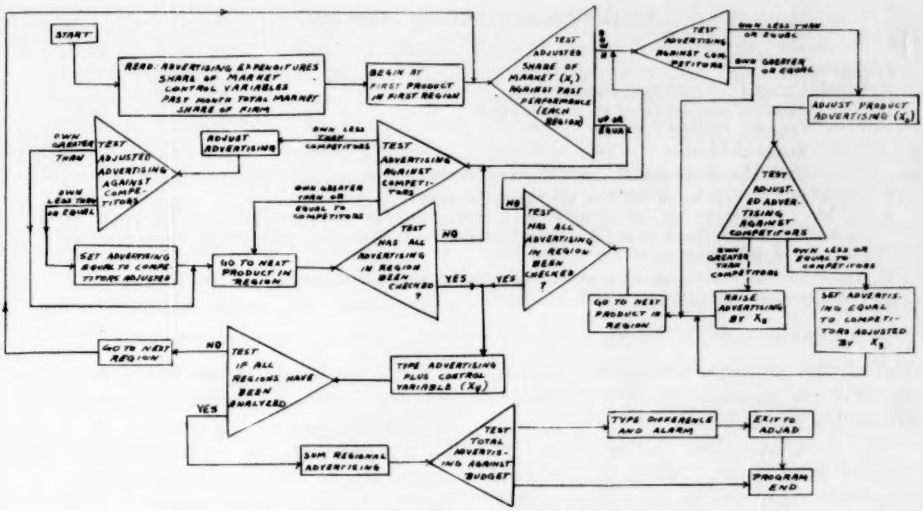
START
READ: ADVERTISING EXPENDITURES
SHARE OF MARKET
CONTROL VARIABLES
PAST MONTH TOTAL MARKET
SHARE OF FIRM
BEGIN AT FIRST PRODUCT IN FIRST REGION
TEST ADJUSTED SHARE OF MARKET (X_1) AGAINST PAST PERFORMANCE (EACH REGION)
UP OR EQUAL
TEST ADVERTISING AGAINST COMPETITORS
OWN LESS THAN OR EQUAL
OWN GREATER OR EQUAL
ADJUST PRODUCT ADVERTISING (X_2)
TEST ADJUSTED ADVERTISING AGAINST COMPETITORS
OWN GREATER THAN
OWN LESS THAN OR EQUAL
ADJUST ADVERTISING
OWN LESS THAN COMPETITORS
TEST ADVERTISING AGAINST COMPETITORS
OWN GREATER THAN OR EQUAL TO COMPETITORS
TEST ADJUSTED ADVERTISING AGAINST COMPETITORS
OWN GREATER THAN COMPETITORS
OWN LESS OR EQUAL TO COMPETITORS
SET ADVERTISING EQUAL TO COMPETITORS ADJUSTED
GO TO NEXT PRODUCT IN REGION
TEST HAS ALL ADVERTISING IN REGION BEEN CHECKED ?
NO
YES
RAISE ADVERTISING BY X_3
SET ADVERTISING EQUAL TO COMPETITORS ADJUSTED BY X_3
GO TO NEXT REGION
TEST IF ALL REGIONS HAVE BEEN ANALYZED
TYPE ADVERTISING PLUS CONTROL VARIABLE (X_4)
SUM REGIONAL ADVERTISING
TEST TOTAL ADVERTISING AGAINST BUDGET
TYPE DIFFERENCE AND ALARM
EXIT TO ADJAD
PROGRAM END

FIG. 1. Advertising Flow Diagram. Control variables shown as $X_1 - X_4$

set by the planning program. If advertising is too low, it is raised to meet the competitor's adjusted advertising. Only if "share of market" has fallen is there a chance that the rote program will raise advertising above the competition. This type of behavior should not really be too surprising in view of the satisfying type of goals explicated previously. The planning program, however, can initiate aggressive action if this is deemed necessary.

Such action can be taken directly through setting a "dollar amount of advertising" control variable greater than zero (X_4 on the flowsheet).

Finally, the program checks "desired advertising" against the amount budgeted. If the amount desired is too large, the program recognizes this by producing an "alarm" output, and computing the amount "desired advertising" exceeds the budget. This output does not go to the financial officer. Instead it goes into the forecasting program, where its effect is to reduce forecasted sales for the next month, and cues the evocation of the "adjust advertising" program.

The outputs of this program will in general be of two sorts: (1) To leave advertising unchanged, or (2) to alter advertising upward to meet competitor's upward raises of this variable. The essence of this mechanism is very similar to an economist's kinked demand curve: it will raise advertising to meet competition, but not lower advertising if competitors lower theirs.

The interested reader can obtain a clear picture of the complexity of outputs which this program allows by setting various control variables negative or null and working through the qualitative behavior of the program, as was done above for the case when all the control variables are greater than zero.

As described above, the "adjust advertising" program shown in Figure 2 is invoked whenever the advertising decision program requires more money to be spent on advertising than the group (or the finance manager) wants to spend.

The "adjust advertising" program first sets up a list of all areas in which the firm's share of market has risen. From the "desired advertising" in each such area it subtracts an amount set by the planning program, testing after each subtraction to see if desired advertising now equals or is less than budgeted. If more money remains to be

FIG. 2. "Adjust Advertising" Flow Outline

1. Start
2. Read: Desired Advertising, each region.
   Amount desired total exceeds budget.
   Control Variable (C 50)
   Share of Market
   Past Month Regional Total Market Share
3. Determine in which regions market share has risen.
4. Subtract C 50 from desired advertising in each region in which market share has risen. After each subtraction, check to see if amount budgeted minus desired advertising is now positive. If so, go to 6. If not, go to 5.
5. Apportion the remaining amount to be cut in 4 parts, cut this much from each region.
6. Program end.

deleted, the program arbitrarily cuts one-fourth of the amount left from the desired advertising for each area.

The program then outputs the new "desired advertising," which is now equal to or less than the amount budgeted for advertising on the product under consideration.

Price determination program

The bulk of the mechanisms used in this program are analogous to those used in the advertising program. Two differences should be recorded.

First, the program can follow price cuts and price increases made by the competition. Whether it does or not depends, of course, on the size of the price difference, the sensitivity of the program to price differences (set by control variables), and how "share of market" has moved for the firm (see Figure 3).

Second, the program also contains a standard cost mechanism. The standard cost mechanism, referred to in the flow sheet simply as "cost," is a markup variable. That is, the "costs" used in the program represent direct costs with a percentage

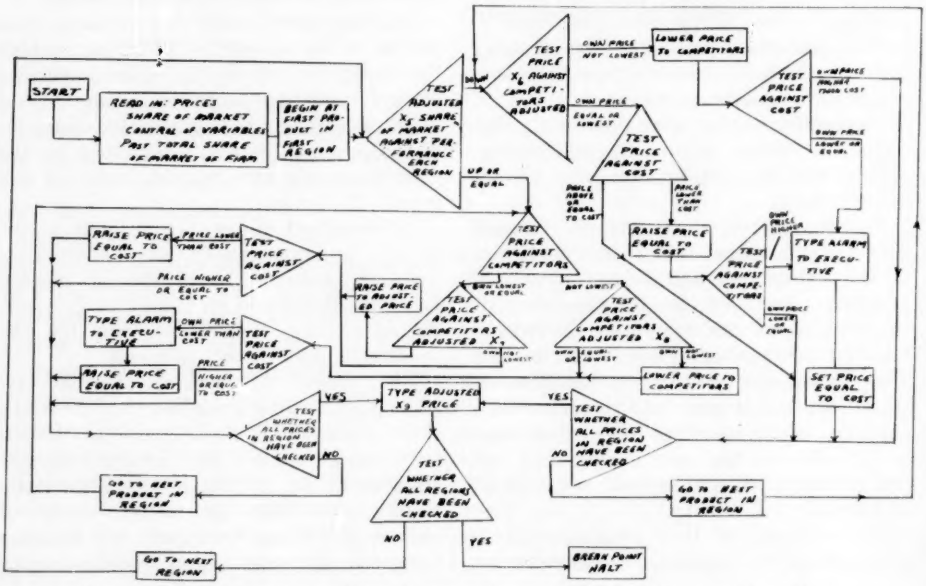
START
READ IN: PRICES
SHARE OF MARKET
CONTROL OF VARIABLES
PAST TOTAL SHARE OF MARKET OF FIRM
BEGIN AT FIRST PRODUCT IN FIRST REGION
TEST ADJUSTED X_9 SHARE OF MARKET AGAINST PERFORMANCE EACH REGION
UP OR EQUAL
TEST PRICE X_6 AGAINST COMPETITORS ADJUSTED
OWN PRICE NOT LOWEST
LOWER PRICE TO COMPETITORS
OWN PRICE EQUAL OR LOWEST
TEST PRICE AGAINST COST
TEST PRICE AGAINST COST
OWN PRICE HIGHER THAN COST
OWN PRICE LOWER OR EQUAL
PRICE ABOVE OR EQUAL TO COST
PRICE LOWER THAN COST
RAISE PRICE EQUAL TO COST
TEST PRICE AGAINST COMPETITORS
TEST PRICE AGAINST COMPETITORS
OWN PRICE HIGHER
TYPE ALARM TO EXECUTIVE
PRICE LOWER THAN COST
PRICE HIGHER OR EQUAL TO COST
TEST PRICE AGAINST COST
RAISE PRICE TO ADJUSTED PRICE
OWN LOWEST OR EQUAL
NOT LOWEST
TEST PRICE AGAINST COMPETITORS ADJUSTED X_9
TEST PRICE AGAINST COMPETITORS ADJUSTED X_9
OWN PRICE LOWER OR EQUAL
TYPE ALARM TO EXECUTIVE
OWN PRICE LOWER THAN COST
PRICE HIGHER OR EQUAL TO COST
TEST PRICE AGAINST COST
RAISE PRICE EQUAL TO COST
LOWER PRICE TO COMPETITORS
SET PRICE EQUAL TO COST
YES
TYPE ADJUSTED X_9 PRICE
YES
TEST WHETHER ALL PRICES IN REGION HAVE BEEN CHECKED
NO
TEST WHETHER ALL PRICES IN REGION HAVE BEEN CHECKED
NO
GO TO NEXT PRODUCT IN REGION
GO TO NEXT PRODUCT IN REGION
TEST WHETHER ALL REGIONS HAVE BEEN CHECKED
NO
YES
GO TO NEXT REGION
BREAK POINT HALT

FIG. 3. Price Determination Flow Diagram. Control variables shown as $X_5 - X_9$

markup. The program will not allow price to fall below this "cost" under any circumstances. If competitors are selling a product at a price sufficiently less than this "cost," the program lowers price to "cost," and outputs an alarm to the planning program.

The role of this particular alarm to the planning program is clear. It exerts pressure for the planning program to (1) lower the percentage markup, (2) put pressure on the advertising program to raise advertising, and (3) increase the search for new (better) products.

Case sales forecasting

The case sales forecasting program has a fairly simple appearance because sales are well structured by the game. The cyclical pattern is rigidly given; thus numbers appear where otherwise control variables would possibly enter.

The program predicts sales five and six months ahead, based on the cyclical pattern, present sales, and share of market movements. The mechanisms are clearly outlined in the flow sheet (see Figure 4, Case Sales Forecasting Flow Diagram). Notice that the program also adjusts forecasts for insufficient advertising effects.

Probably the most interesting aspect of this program is the tremendous amount of uncertainty it absorbs. Because the factory needs stable forecasts on which to base production, shipping, and raw materials orders, the program makes no changes in predicted sales for five months save for a possible downward adjustment due to low advertising. This may seem unreal; it may seem that "nobody would do this." The only defense for it is that this is precisely the mechanism used by the teams observed.

Distribution expenditure program

This program, which determines distribution expenditures on the basis of the cyclical pattern and past month case sales, is represented in Figure 5. One team which was observed at first used advertising expenditures instead of past month case sales, but later switched to using past month case sales "because they felt it was better." The question of how people evaluate alternative heuristics is not explored further in this paper, although it is clearly an interesting one. The program does not respond in any manner to competitors during the peak months; during the low sales months (eight through two) it can lower distribution expenditures if competitors have set their expenditures sufficiently below the firm's own previous level.

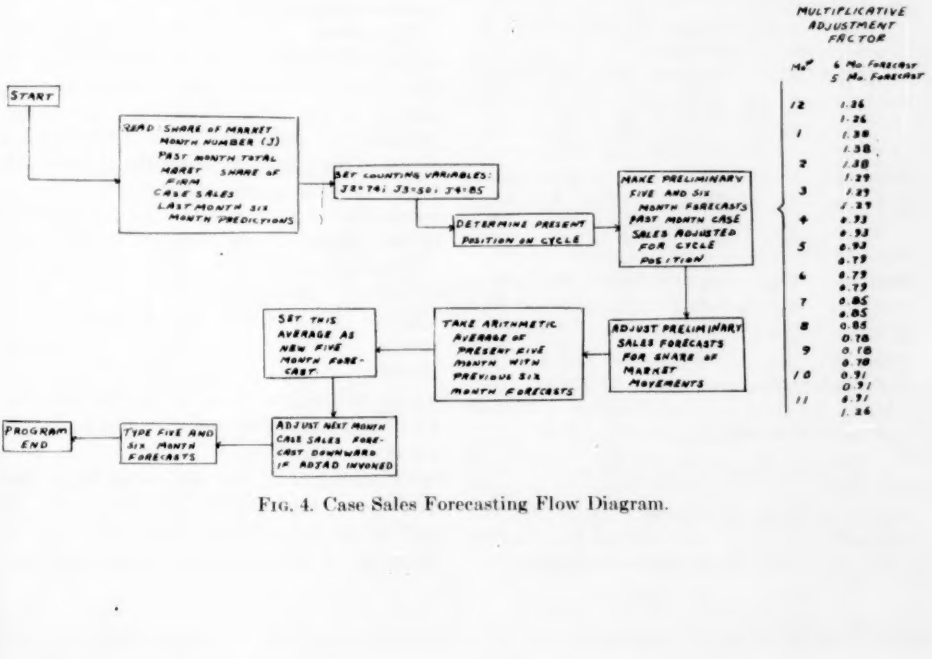
START
READ SHARE OF MARKET
MONTH NUMBER (J)
PAST MONTH TOTAL MARKET
SHARE OF FIRM
CASE SALES
LAST MONTH SIX MONTH PREDICTIONS
SET COUNTING VARIABLES: J2=74; J3=50; J4=85
DETERMINE PRESENT POSITION ON CYCLE
MAKE PRELIMINARY FIVE AND SIX MONTH FORECASTS PAST MONTH CASE SALES ADJUSTED FOR CYCLE POSITION
ADJUST PRELIMINARY SALES FORECASTS FOR SHARE OF MARKET MOVEMENTS
TAKE ARITHMETIC AVERAGE OF PRESENT FIVE MONTH WITH PREVIOUS SIX MONTH FORECASTS
SET THIS AVERAGE AS NEW FIVE MONTH FORECAST
ADJUST NEXT MONTH CASE SALES FORECAST DOWNWARD IF ADJAD INVOKED
TYPE FIVE AND SIX MONTH FORECASTS
PROGRAM END
MULTIPLICATIVE ADJUSTMENT FACTOR
Mo. | 6 Mo. Forecast | 5 Mo. Forecast
12 1.26 1.26
1 1.38 1.38
2 1.38 1.29
3 1.29 1.29
4 0.93 0.93
5 0.93 0.79
6 0.79 0.79
7 0.85 0.85
8 0.85 0.78
9 0.78 0.78
10 0.91 0.91
11 0.91 1.26

FIG. 4. Case Sales Forecasting Flow Diagram.

FIG. 5. Distribution Expenditure Flow Outline

1. Start
2. Read: Month Number
   Product and Policy Control Variables
   Past Month Distribution Expenditures
   Case Sales
3. Test: Are we in high part of sales cycle?
   a) No—Go to 4
   b) Yes—Go to 8
4. Adjust Distribution Expenditures (X_{12})
5. Test: Adjusted distribution against competitors
   a) Higher—Leave distribution the same, go to 10
   b) Lower—Go to 6
6. Adjust Multiplicative Factor by X_{10}
7. Set distribution equal to past month case sales times multiplicative factor plus X_{11}, go to 10
8. Adjust Multiplicative Factor by X_{13}
9. Set distribution equal to past month case sales times multiplicative factor plus X_{14}
10. Type Distribution
11. Program End.

TESTING OF THE SYSTEM

Before discussing the implications which can be drawn from the above system, we shall consider the validity of the cognitive processes represented by the above programs.

Two different methods of testing were used. The first was simulation of actual decisions made by various firms.

TABLE 1
ADVERTISING SIMULATION
(In thousands of dollars)

		Predicted	Real	Random	Naive
Product A	E	19.8	20	308	26
	S	19.5	20	319	20
	C	64.0	20	848	20
	W	28.0	30	563	30
Product B	E	380	330	481	380
	S	300	300	1	300
	C	553	500	588	550
	W	507	500	276	500
Product C	E	51	50	267	50
	S	0	90	430	90
	C	204	50	161	50
	W	158	90	97	90

(1) Σ (Real-Predicted)^2 = 43,737.25
$\Sigma\Delta_1^2$ = 25,399.35
(2) Σ (Real-Random)^2 = 1,847,199.0
$\Sigma\Delta_2^2$ = 532,346.92
(3) Σ (Real-Naive)^2 = 5,036.0
$\Sigma\Delta_3^2$ = 4,099.67

H_0: (1) = (2) $\hat{F}$ = 20.96 reject H_0 (α = .05) for $F_{11,11}$ = 2.82
H_0: (1) = (3) $\hat{F}$ = 6.20 reject H_0.

Tables 1 to 4 report the results of these simulations. It is obvious from examination of the figures that the simulation results come "fairly close" to the actual decisions the firms made.

The question which arises is whether this supposed closeness is real or not. We propose a simple-minded test to attempt to answer this question.

This test is based on an alternative model. The alternative model asserts that the actual decisions can be better explained by assuming the numbers used by the firms were generated randomly.

The reason for doing this is simple. The entire discussion of this paper is based on the assertion that the decision-making process of the firms studied is nonrandom. If, however, randomly drawn numbers come closer to the actual decisions than the results of the theoretical model described above, it seems clear that we should become suspicious of the validity of the proposed model.

Therefore, a list of random numbers with a uniform distribution were drawn from Duncan (1955, p. 633) and set down in the column headed "Random." A second alternative model is to say that next period's results will be the same as the last period's. The predictions this model would make are listed in the "Naive" column. This is a fairly strong test, and we would hope that our theory can account for the results as well as the naive model does. The point is, of course, that such a naive model does not

TABLE 2
PRICING SIMULATION
(In Dollars per Case)

		Predicted	Real	Random	Naive
Product A	E	6.10	6.17	7.73	5.97
	S	6.17	6.27	4.24	6.07
	C	6.10	6.05	6.08	6.00
	W	6.14	6.13	2.20	6.04
Product B	E	5.90	5.92	9.15	5.78
	S	5.90	5.98	6.49	5.78
	C	5.90	5.82	3.66	5.75
	W	6.00	6.05	2.21	5.90
Product C	E	6.48	6.22	9.21	6.38
	S	6.30	6.29	3.47	6.39
	C	6.58	6.48	3.47	6.48
	W	6.30	6.36	6.40	6.42

(1) Σ (Real-Predicted)^2 = 0.1445
$\Sigma\Delta_1^2$ = 0.0955
(2) Σ (Real-Random)^2 = 76.8947
$\Sigma\Delta_2^2$ = 20.7779
(3) Σ (Real-Naive)^2 = 0.2268
$\Sigma\Delta_3^2$ = 0.0384
H_0: (1) = (2) $\hat{F}$ = 217.57 reject H_0 (α = .05), for $F_{11,11}$ = 2.82
H_0: (1) = (3) $\hat{F}$ = 2.49 cannot reject H_0.

help in explaining why existing decision patterns exist, while the theoretical model presented above does (Savage & Deutsch, 1960).

To test the closeness of fit, the difference between the three models and the actual decisions are tabulated, and from these numbers the three relevant variances are calculated. See Duncan (1955) for the F tables used and for the graph of the operating characteristics curve for the test (p. 405).

TABLE 3
CASE SALES—FORECASTING SIMULATION
(Sales in thousands)

	Predicted	Real	Random	Naive
Month 19	1,855	2,891	6,539	2,669
Month 20	2,531	2,272	8,764	2,891
Month 21	1,949	2,183	861	2,272
Month 22	2,119	2,286	852	2,183
Month 23	2,246	2,507	8,986	2,286
Month 24	2,326	1,893	4,230	2,507

(1) Σ (Real-Predicted)^2 = 1,478,632
$\Sigma\Delta_1^2$ = 526,616
(2) Σ (Real-Random)^2 = 106,697,018
$\Sigma\Delta_2^2$ = 28,128,528
(3) Σ (Real-Naive)^2 = 876,812
$\Sigma\Delta_3^2$ = 295,242
H_0: (1) = (2) $\hat{F}$ = 53.41 Reject H_0 (α = .05) for $F_{5,5}$ = 5.05
H_0: (1) = (3) $\hat{F}$ = 1.78 Cannot reject H_0.

TABLE 4
DISTRIBUTION EXPENDITURE SIMULATION

		Predicted	Real	Random	Naive
Product A	E	151	208	258	120
	S	143	116	453	120
	C	147	50	113	120
	W	141	201	897	120
Product B	E	87	61	88	90
	S	90	89	60	90
	C	87	151	67	90
	W	88	40	50	90
Product C	E	28	0	77	20
	S	35	20	27	40
	C	31	38	54	40
	W	30	0	11	40

(1) Σ (Real-Predicted)^2 = 25,654
$\Sigma\Delta_1^2$ = 8,388.67
(2) Σ (Real-Random)^2 = 619,535
$\Sigma\Delta_2^2$ = 454,564.25
(3) Σ (Real-Naive)^2 = 28,688
$\Sigma\Delta_3^2$ = 10,591.67
H_0: (1) = (2) $\hat{F}$ = 54.19 Reject H_0 (α = .05), $F_{11,11}$ = 2.82
H_0: (1) = (3) $\hat{F}$ = 1.26 Cannot reject H_0.

The results of these tests are reported at the bottom of each table. In all cases, our model comes closer to the actual decisions than does the random number model. Our model does as well as the "naive" model in all cases except the advertising simulation.

This type of simulation, however, raises another question: could such relatively good fits have been obtained by varying the control parameters to fit the one case? In other words, are these programs spurious in the sense that the control parameters would have to be varied for each decision period to achieve sensible decisions?

Rather than attempt to run simulations over a long period of time, a slightly different test was proposed. This test was to let the program be used by one team which actually was engaged in playing the business game and to allow them to reject the program if they felt that its decisions were unsuitable. If the program was used, they would be free to alter the control parameters as they saw fit, and as much as they saw fit. This was actually done. For a detailed report of the effects this had on the team, see Haines, Heider, and Remington (1960).

The team used the program for six months (a month is the decision unit). The team accepted the program, used it, and continued to use it as long as the computer in which the program was run was available to the team. During this time the control parameters remained unchanged, except for the introduction of a new product. At this time some (but not all) control parameters were altered. Also, the value of one control parameter in the pricing program (X_9 on Figure 3) was made contingent upon whether or not inventory run-outs had occurred in the past month. We can conclude that once set, the control parameters are exceedingly stable.

This test had the additional feature of testing the reality of the program by throwing it into competition with humans. At any time the team could have dropped the program, but they did not do this. The conclusion is that the program is actually useful in the "real world" of the game. The team members believed that use of the program aided them because it forced them to think in terms of strategies, not month-to-month decisions.

As part of this test, a modified Turing test was performed. The competitors of the team were asked whether they could tell in what month the firm had begun to use the program (this fact previously being kept secret). They were unable to do so. This is not really a very strong test, as almost any adaptive program would give this kind of result.

There is no evidence to deny the proposition that the program does appear to have captured the essence of the actual rote (i.e., nonlearning) part of the decision-making process. It must be kept in mind, however, that probably none of the tests used have great power.

THE GAME AND THE REAL WORLD

The program presented above represents a theory of how management game players make decisions. This theory consists of a structure of salient facts about the environment, through which the program processes the appropriate data from the real world in its decision-making. In this it resembles the metaplan of Miller, Galanter, and Pribram (1960).

The structure itself, one may conjecture, arises out of a search for and recognition of patterns in the real world. For example, in the pricing program the list of prices which is set up is composed of four areas, nine products per area: a structural aspect of the game.

Now, if we compare this with a study of how such decisions are made in the real world, striking similarities occur (Cyert, March, & Moore, 1961; Cyert & March, in press).

An analogous decision-making procedure appears in the Cyert, March, and Moore study of a department store referred to above. The case sales forecasting programs are essentially the same in structure, and in the heuristics used. Further, the pricing process outlined in the department store study follows an almost exactly similar pattern to the theory outlined above for management game players. In both programs, a standard cost is a key variable in the pricing decision process; the type of attention paid to competitors' prices is very similar also.

The differences in the two programs seem to come from structural differences in the real worlds under consideration. This is not surprising if we believe the underlying postulates to be the same, for then environmental differences would cause different actual behavior. For example, the department store study contains a subprogram to deal with sale pricing; this is absent in the game program. The reason for this is simply that "sales" are not allowed in the Carnegie Tech Management Game.

RELATION TO ORGANIZATION THEORY

The general model developed here from observations of the "real" game world provides some interesting illustrations of some propositions in organization theory.

First, the general model built here is not just epicyclic in nature. The higher level affects the lower level program in only a few places, through the control variables. The essence of the mechanism used has been described by March and Simon (1958,

p. 150) as follows: "Limitation of high level action to the recombination of programs, rather than the detailed construction of new programs out of small elements, is extremely important. . ."

Similarly, organization theory predicts that "in organizations where various aspects of the whole complex problem are being handled by different individuals and different groups of individuals, a fundamental technique for simplifying the problem is to factor it into a number of nearly independent parts, so that each organizational unit handles one of these parts and can omit the others from its definition of the situation." The model presented here of marketing decision-making precisely illustrates this contention.

Furthermore, we are here faced with a relatively stable environment, namely, the rules of the game. Thus it is not surprising that there is only limited communication between the specialized subprograms. The co-ordination which does exist between the subprograms is largely co-ordination based on pre-established plan (March & Simon, 1958, p. 143, 150). Moreover, this communication is measurable; in theory, at least, more or less communication could be used and the effect of this discovered by experiment.

The above program also illustrates the structure of programs for handling recurrent events, and the organizational aspects of such programs.

It is obvious that the program presented in this paper, which is concerned largely with programs to respond to sequences of situations requiring short-run adaptive behavior, exhibits the form hypothesized to exist in organizations in general. This in itself is an interesting fact. The question arises as to whether such a fact can be used to push into the area of testing hypotheses in organizational theory. As of yet, no statistical answer to the question has been proposed.

REFERENCES

Clarkson, G. P. E. Trust investment: a study in decision making. Behavioral Theory of the Firm Working Paper No. 24, Carnegie Institute of Technology, 1960.

Cohen, K. J., Cyert, R. M., Dill, W. R., Kuehn, A. A., Miller, M. H., Van Wormer, T. A., & Winters, P. R. The Carnegie Tech management game. *J. Business*, 1960, 23.

Cyert, R. M., & March, J. G. *Behavioral theory of the firm.* Englewood Cliffs, N. J.: Prentice-Hall, in press.

Cyert, R. M., March, J. G., & Moore, C. G. A specific price and output model. Behavioral Theory of the Firm Working Paper No. 30, Carnegie Institute of Technology, 1961.

Duncan. *Quality control and industrial statistics.* Homewood, Ill.: Irwin, 1955.

Fellner, W. Average cost pricing and the theory of uncertainty. *J. polit. Economy*, 1948, 56, 249–255.

Haines, G. H., Heider, F., & Remington, D. The computer as a small group member. Behavioral Theory of the Firm Working Paper No. 28, Carnegie Institute of Technology, 1961.

March, J. G. & Simon, H. A. *Organizations.* New York: Wiley, 1958.

Miller, G. A., Galanter, E., & Pribram, K. H. *Plans and the structure of behavior.* New York: Holt, 1960.

Newell, A., Shaw, J. C., & Simon, H. A. Elements of a theory of human problem solving. *Psychol. Rev.*, 1958, 65, 151–166.

Newell, A., & Simon, H. A. The simulation of human thought. RAND Corporation Report P-1734, 1959.

Savage, I. R., & Deutsch, K. W. A statistical model of the gross analysis of transactions flows. *Econometrica*, 1960, 28, 552–570.

(Manuscript received April 24, 1961)

Computer Research in Psycholinguistics: Towards an Analysis of Poetic Language,[1] *Thomas A. Sebeok and Valdis J. Zeps, Center for Advanced Study in the Behavioral Sciences.*

The report which follows outlines the programs used to date in the project identified above, and concerns the automatic construction of linguistic lists, which may be regarded as essentially procedures for dictionary building (although some may be designed for rather special application). This

[1] This research was supported in large part by National Science Foundation Grants Nos. G5556 and G13017, made to the Indiana University Research Center in Anthropology, Folklore, and Linguistics. We record with appreciation the assistance of the Indiana University Research Computing Center staff, and the opportunity for a year's residence at the Center for Advanced Study in the Behavioral Sciences. For background, see Osgood and Sebeok (1954), Saporta and Sebeok (1959), and Sebeok (1960, 1961a).

undertaking was motivated by the necessity for disciplining (arraying, displaying) our data in a fashion which would make easy reference possible and thus facilitate further research. The advantages of employing computers for the construction of lists are known well enough to require no comment (cf., however, Painter, 1960).

Those of our lists which we deemed of more than passing interest have appeared in book form (Sebeok & Zeps, 1961). Basic to these is the Concordance, accompanied by several of its transforms. Of the latter, we may mention the Tergum, which consists of a reverse listing of all forms, to which are added several features intended to facilitate the study of derivation.

The investigation was based on a corpus of verse texts, assembled from a variety of sources. The texts were pre-edited (Sebeok & Zeps, 1958), punched on cards, and assembled in the IBM 650 electronic data processing system, equipped with four magnetic tape units and three index accumulators, as well as peripheral equipment. All programs and operator's instructions are on deposit at the Indiana University Research Computing Center. The brief descriptions which follow are offered to enable the reader to visualize the input and output formats. The number of records that the system can handle does not exceed 48,000 80-digit records, i.e., the capacity of two tapes.

1.1 Segmenting and enumerating program

Basic to the construction of our Concordance, and underlying its transforms, is a segmenting and enumerating routine (Figure 1) and the alphabetic sort which follows this. The first routine utilizes a card input of the following format: columns 1 and 2 contain a numerical code identifying the collection; columns 3, 4, and 5 contain a numerical code identifying the text within the collection. Column 5 has a superimposed 12-punch. Columns 43–72 contain the text, punched consecutively in alphanumeric characters. Of these, the characters "8" and "9" (hereafter referred to as "shifters") have a special significance: "8" has no other function but to signal the beginning of a stretch containing two alphanumeric columns of an auxiliary code, and "9," three columns of the same.

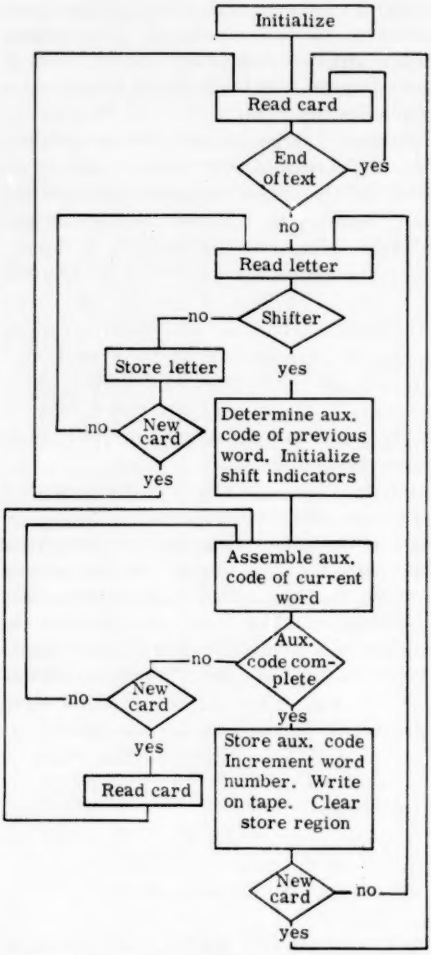

FIG. 1. Flow Chart of the Segmenting and Enumerating Routine.

The routine calls for the use of an IBM 650, one tape unit, and index accumulators.

The output appears as a sequence of 70-digit records on the tape. Of these, the first 40 contain the alphanumeric record of the stretch to the left of the shifter (in our

case, the phonemic shape of a stem or an ending); the next 10 contain the auxiliary code of the previous record; the next 10, the auxiliary code appended to the record itself, i.e., the two or three columns to the right of the shifter; and the next 10, the identification section. In the identification section, the first five digits replicate columns 1–5 of the input card; the remaining digits contain the identification of the record within the text, assigned by the routine mechanically.

1.2 Alphabetic sort

For this operation, we utilize the "IBM 650 Tape Sorting Program—SORT II." Its input can be any tape, or, in constructing our library tape, the output of the previous routine. In the latter case, we select a field of 50 digits, namely, the phonemic record and the identification section as the basis for the sort. For other purposes—for example, to resort back in text order, or to subsort by form class (identified by digits 3 and 4 of the post-auxiliary code section)—a different field assignment is made.

1.3 Assignment of numerical equivalents

To further facilitate processing, an arbitrary numerical equivalent was assigned to each record; records with identical alphanumeric sections were assigned identical numerical equivalents.

This program operates on the output of the alphabetic sort routine, and utilizes two tape units. The resulting tape consists of 80-digit records, as in the output of the segmenting and enumerating routine (Figure 2), except that the records are alphabetized, and a numerical equivalent of 10 digits is appended to each record.

The output of the three routines described (1.1–2–3) constitutes our basic library tape. The library tape can in turn be resorted according to different specifications; the tape can be printed out on cards for operations not requiring the use of the IBM 650; blocks of records can be retrieved by a search routine; and individual records can be added, erased, or corrected by means of a correction routine.

The remaining programs (2.–3.) do not form part of the basic library tape cycle, and were designed for special purposes.

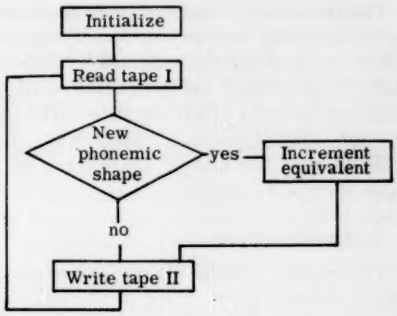

FIG. 2. Flow Chart of the Numerical Equivalents Routine.

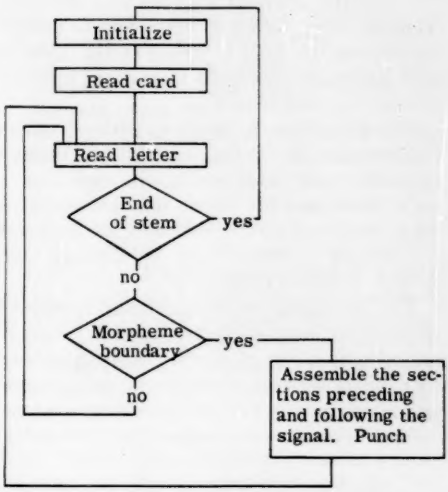

FIG. 3. Flow Chart of the Derivation Routine.

2. Derivation routine

Named after its initial application in a study of derivational suffixes, this routine (Figure 3) utilizes a card input and the basic 650. The routine operates on the alphanumeric (phonemic) section of each record, punched on columns 43–62, senses some predetermined signal (such as a morpheme boundary), and prints out in two different fields of a card both sections separated by that signal.

The routine is not limited in its application to derivational studies; by specifying some different signal for the separation of the two fields to be printed on the output card, the relations between other linguistic units, such as the distributions, transitional probabilities, and the like, of phonemes, can be studied.

3. Reversal routine

This routine (Figure 4) uses a card input, simply reversing the order of the entries in specified sections of the input card and printing the output on a new card.

In combination with the derivation routine, the reversal routine underlies our Tergum. The routine is necessary to permit sorting on the final portions of the records (not normally accessible to sorting, because of their varying length).

In addition to the routines utilized in the construction of our Concordance, additional counting and abstracting routines have been developed for future use, primarily in syntactical and metrical studies, as discussed by Sebeok (1959), and exemplified by Sebeok & Zeps (1959).

The increasing use of automatic machines as tools for linguistic investigations (including certain types of literary investigations) was the topic of an international symposium organized by the Centro per l'automazione dell'analisi letteraria, held at the University of Tübingen in November, 1960. We carried away two dominant impressions from this three-day gathering: first, that the mechanical processing of linguistic data involves a family of subdisciplines which are at present advancing without adequate co-ordination. Thus we feel that the subdiscipline of mechanical translation—perhaps the most affluent because potentially the most utilitarian member of this family—is closely related to the techniques of automatic analysis of texts, as well as to indexing and abstracting, scanning of stored materials, and the like, all of them vitally connected with the development of character and pattern recognition devices, and with speech analysis and synthesis (Sebeok, 1961b).

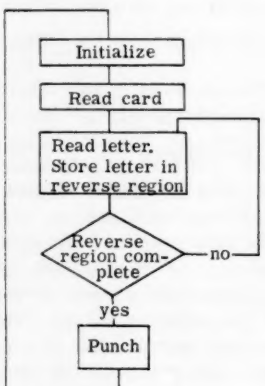

FIG. 4. Flow Chart of the Reversal Routine.

Second, we now are convinced, from our own experience and the accounts of others, that the chief and ultimate value of such researches with information processing machines for linguistics as such will derive less from economic or other practical gains (important though these may be) than from their stringent demand for precision and hence maturing effect on linguistic methodology.

REFERENCES

Osgood, C. F., and Sebeok, T. A. (Eds.), *Psycholinguistics: a survey of theory and research problems. Suppl. J. abnorm. soc. Psychol.*, 1954, 49.

Painter, J. A. Computer preparation of a poetry concordance. *Comm. Assoc. Computing Machinery*, 1960, 91–95.

Saporta, S., and Sebeok, T. A. Linguistics and content analysis. I. de S. Pool (Ed.), *Trends in content analysis*, Ch. 4. Urbana: Univ. of Illinois Press, 1959.

Sebeok, T. A. Approaches to the analysis of folksong texts. *Ural-Altaische Jahrb.*, 1959, 31, 392–399.

Sebeok, T. A. (Ed.), *Style in language*. New York, London: Technology Press of M. I. T. & John Wiley and Sons, 1960.

Sebeok, T. A. Notes on the digital calculator as a tool for analyzing literary information. *Poetics*. Warsaw: Literary Research Institute, Polish Academy of Sciences, 1961(a).

Sebeok, T. A. Summation by chairman, Session 3: Current Research. In H. P. Edmundson, (Ed.), *Proceedings of the national symposium on machine translation*. Englewood Cliffs, N. J.: Prentice-Hall, Inc., 1961 (b), 193-194.

Sebeok, T. A., and Zeps, V. J. An analysis of struc-

tured content, with application of electronic computer research in psycholinguistics. *Lang. and Speech*, 1958, 1, 181–193.

Sebeok, T. A., and Zeps, V. J. On non-random distribution of initial phonemes in Cheremis verse. *Lingua*, 1959, 8, 370–384.

Sebeok, T. A., and Zeps, V. J. *Concordance and thesaurus of Cheremis poetic language. Janua Linguarum Series Maior* 8. The Hague: Mouton & Co., 1961.

(Manuscript received May 19, 1961)

COMPUTER PROGRAM ABSTRACTS

LGP 30 Program for Significance Tests of Difference Between Means, *Richard A. Cabot, The University of Michigan.* **(CPA 47)**

Description: The program tests for the significance of the difference between two means. Different procedures are used for small or large sample data ($N_S \leqslant 30 < N_L$) and for correlated or independent means. Raw data may be positive or negative three-digit numbers with sample size less than 1,001 and the difference between means less than 1,024. For a small sample test of independent means $N_1S_1^2 + N_2S_2^2$ must be less than 16,384. The program requires 23 tracks of storage.

A detailed description and LGP 30 listing of this program has been deposited with the ADI Auxiliary Publications Project, Photoduplication Service, Library of Congress, Washington 25, D. C.

Order Document 6722 and remit $5.00 for photoprints, or $2.25 for 35mm microfilm. Advance payment is required. Make checks or money orders payable to: Chief, Photoduplication Service, Library of Congress.

Sequential Dependencies Analysis on IBM 704, *Myron L. Braunstein, Cornell Aeronautical Laboratory, Inc., Buffalo 21, New York.* **(CPA 48)**

Description: The program is designed to measure the redundancy in sequences of stimuli or responses. The stimuli, responses, and other data are coded into a series of single digits, a different digit representing each alternative response. The frequency with which each pattern of the digits occurs is counted for groups of one to J digits, where J is the highest order of redundancy being computed. Each frequency, n_i, is multiplied by log n_i, and the redundancy is computed by the formula

$$R_j = R_{j-1} - \frac{\sum p_i \log p_i}{\log m^j}$$

where R_j is the redundancy for sequences of length j and m is the number of alternative stimuli or responses. Output for each order of redundancy includes a list of all possible patterns with frequency of occurence and $n_i \log n_i$, $\sum p_i \log p_i$, $\sum p_i \log p_i/m^j$ and R_j.

Computer: IBM 704. **Program Language:** FORTRAN.

A detailed write-up and a FORTRAN II listing have been deposited with the ADI Auxiliary Publications Project, Photoduplication Service, Library of Congress, Washington 25, D. C. Order Document 6720 and remit $1.25 for photoprints, or $1.25 for 35mm microfilm. Advance payment is required. Make checks or money orders payable to: Chief, Photoduplication Service, Library of Congress.

Analysis of Experimental Designs by Means of Randomization, a Univac 1103 Program, *Frank B. Baker and Raymond O. Collier, University of Wisconsin and University of Minnesota.* **(CPA 49)**

Description: The empirical distribution of the F ratios resulting from either a randomized block or split-plot design can be obtained through randomization. The observations are randomized according to the principle of the design. The analysis of variance is performed and the obtained F ratios punched in cards. The first four powers of the F ratios are computed and accumulated in memory. The randomization and analyzing process is continued until 1,000 samples have been completed. The obtained value of the F ratios can then be tested against the empirically derived distribution. The first four moments can be computed and compared with known distributions. Options are available for use of several different mean square errors in each design.

In the designs there are: I blocks, J treatments, K replicates, and N observations per cell. The following restrictions apply simultaneously: $1 \leqq n \leqq 3$, $(I + 1)(J + 1) \leqq 96$, $N(I)(J)(K) \leqq 5631$, $(I + 1)(J + 1)(K + 1) \leqq 4096$, and the observations are three-digit integers.

The input-output medium is standard 80-column punched cards. Floating-point arithmetic is used to perform the analysis of variance.

A detailed description of this program has been deposited with the ADI Auxiliary Publications Project, Photoduplication Service, Library of Congress, Washington 25, D. C.

Order Document 6721 and remit $1.25 for photoprints, or $1.25 for 35mm microfilm. Advance payment is required. Make checks or money orders payable to: Chief, Photoduplication Service, Library of Congress.

An IBM 709 Program for Correlation Analysis and General Multiple Regression, *Paul J. Hoffman, Oregon Research Institute, Eugene, Oregon.* **(CPA 50)**

Description: This program computes means, variances, and intercorrelations for any number of sets of data, and optionally, regression coefficients, multiple correlation coefficients, and relative weights for each criterion variable. The program provides for up to 100 variables, predictors plus criteria, and 100 observations per variable. An alternate program will handle a 200 × 75 matrix. The program is based on an iterative predictor selection procedure developed by Horst. It will provide meaningful estimates of multiple R when matrix inversion methods are inapplicable.

Running Time: Principally a power function of the number of variables. Intercorrelations among 100 predictors normally require less than two minutes; regression equations involving 20 predictors may require 30 seconds for each criterion variable. The number of iterations necessary for the solution of the beta coefficients may be controlled by a constant, depending on the degree of accuracy desired.

A writeup has been prepared and is available from Oregon Research Institute. Program decks are also available at nominal cost.

A Bendix G-15D Program for Chi Square for 2 x 2 Contingency Tables, Corrected for Continuity, *Rhea S. Das, Lafayette Clinic, Detroit, Michigan.* **(CPA 51)**

Description: Given the four cell frequencies of a 2 × 2 contingency table, this program computes chi square, corrected for continuity, by the formula

$$\chi^2 = \frac{N(|AD - BC| - N/2)^2}{(A + C)(B + D)(A + B)(C + D)}$$

(Walker and Lev, 1953). The cell frequencies may be entered by typewriter input or paper tape prepared on an off-line Flexowriter. There is no limit on the number of problems to be solved, but the total of the cell frequencies for any one problem must not exceed 2,047. **Computer:** Bendix G-15D, Alphanumeric Typewriter, Flexowriter (optional). **Running Time:** Chi square will be computed, tested for significance, and typed out in 19 seconds for a single problem with typewriter input (excludes type-in time) and in 21 seconds with paper tape input (includes tape-read time). **Comments:** The program is written in machine language, with fixed point single precision arithmetic. Chi square is computed to four decimal places, and the level of significance indicated.

The program write-up, including operating instructions, and the program tape can be obtained from the Bendix Users' Exchange, Bendix Computer Division, 5630 Arbor Vitae Street, Los Angeles 45, California, by requesting Users' Project 585.

Reference:

Walker, H., and Lev, J. *Statistical inference.* New York: Holt, Rinehart & Winston, Inc., 1953.

IBM 650 Program for Analysis of Variance, *F. D. Stockton, Monsanto Chemical Company, Springfield, Mass.* **(CPA 52)**

Description: For a complete factorial design this program computes specified interactions and, optionally, N-way tables, averages, and numbers of summands per entry. These computations are computed from sets of data ordered by up to ten variables, each of which may have up to nine levels. The program, however, can handle no more than 1,600 data entries. The program is believed to be an improvement on existing programs for the IBM 650 in that provision is made for (1) data replication, (2) the specification of interactions to be computed, (3) the optional printing of N-way table data, (4) the accuracy of the interactions and N-way sums, and (5) the inclusion of negative data entries.

Computer: Basic IBM 650, alphabetic device. **Program Language:** SOAP II. **Running Time:** Variable. An example: 81 data entries with three variables (A, B, and C). The A, B, AB, C, AC, BC, and ABC effects took six minutes. **Comments:** The interactions and the N-way sums are given to twenty digits. If these quantities exceed twenty digits, the high-order digits are dropped. Subsequent calculations with those quantities will, therefore, be invalid. Each data entry is a signed ten-digit number. It is suggested that regardless of the location of the decimal, the first digit be put in the sixth column entry; i.e., it is suggested that the entries be five-digit numbers. This restriction of the data accuracy assures that twenty-digit N-way sums or interactions will not be exceeded.

A manual for this program will be published by the American Institute of Chemical Engineers, 25 West 45th Street, New York 36, N. Y., if sufficient interest develops. Reprinted from *Chem. Eng. Prog.* 1961, 57, 2, 80, where it was program 067.

Univac I Program: A General Equation Solver, *E. A. Traynor, DuPont de Nemours & Company, Wilmington 98, Delaware.* **(CPA 53)**

Description: A compiler system has been written that permits the user to write computer instructions in English and algebraic symbols for solving algebraic equations. The compiler pseudo code can handle all the elementary mathematical functions, perform tests and transfer, and provide simple iteration solutions.

Computer: Univac I with I instruction; Univac II in Univac I mode: 10 tapes. **Program language:** C-10. **Running Time:** On Univac I, running time in minutes (T) can be approximated by the formula:

$$T = 1 + \frac{N}{33}$$

where N is the number of statements in the pseudo code listing. Univac II running time is approximately half that of Univac I. **Comments:** The main features of this system, for problems which are applicable to it, are that:

1. No memory layout is required.
2. Input-output controls, listings, and running program are all unified on one magnetic tape.
3. The speed of compile is most attractive. Typical times on Univac I, including tape mounting, are 7.5 minutes for 160 statements compared to 11 minutes for compiling the identical problem with Fortran on the IBM 704.

A manual for this program will be published by the American Institute of Chemical Engineers, 25 West 45th Street, New York 36, N. Y., if sufficient interest develops. Reprinted from *Chem. Eng. Prog.* 1961, 57, 3, 88, where it was program 068.

Burroughs 205 Program for Comparison of Means (Scheffe test), *W. N. Smith, Dow Chemical Company, Freeport, Texas.* **(CPA 54)**

Description: This program compares the averages or means of each of several sets of data using the method of Henry Scheffe (1953). The program calculates the mean and variance for each set, and then pairs the means off to determine if the difference between the means is significant. It checks all possible comparisons of this type. The print out lists the mean and variance of each set, and then lists the pairs that are significantly different, along with the difference. The significance can be based on either 90% or 95% confidence level. The program will handle up to 100 sets of data, each set containing up to 500 data. The program uses fix-float, square root, and a table look-up subroutines.

Computer: Burroughs 205, floating point, card reader (PCC), on-line printer. **Program Language:** Pseudo Code (compiled in machine language). **Running Time:** Varies with the number and size of the sets. About 60 seconds for 10 sets, each containing 30 data. **Comments:** The sets do not have to contain the same number of data.

A manual for this program will be published by the American Institute of Chemical Engineers, 25 West 45th Street, New York 36, N. Y., if sufficient interest develops. Reprinted from *Chem. Eng. Prog.* 1961, 57, 1, 90, where it was program 063.

Reference:

Scheffe, Henry. A method for judging all contrasts in the analysis of variance. *Biometrika*, 1953, 40, 87.

IBM 704 Program for Fisher's F-distribution, *Clark Holloway, Jr. and Walter B. Capp, Gulf Research and Development Company, P. O. Box 2038, Pittsburgh 30, Pennsylvania.* **(CPA 55)**

Description: The F-distribution includes two degrees of freedom, M and N, usually called the numerator degrees of freedom and the denominator degrees of freedom, respectively. F ranges from zero to infinity. The probability P of occurrence of a value equal to or smaller than F is given by the area under the F-distribution curve from zero to F. The calling sequence required for the F-distribution subroutine is:

$$CALL\ FDISTR\ (M, N, F, P)$$

The FORTRAN subroutines LOG and EXP are also required. M and N must be positive integers. Values less than 1 or greater than 32,767 will result in an incorrect calculation. A negative value for F will result in an error halt. If a positive value is given for F, the subroutine will compute P and store the result in location P. If F is given as zero, the subroutine will use P to compute the corresponding value for F, leaving it in location F. In this case, a negative or a zero value for P will result in an error halt. As an example, if the values $M = 2$, $N = 3$, $F = 9.55$ are supplied, the subroutine will calculate $P = 0.95$. If the values $M = 2$, $N = 3$, $F = 0$, $P = 0.95$ are supplied to the subroutine, it will calculate the value $F = 9.55$.

Computer: IBM 704, minimum memory required, no tapes or drum. **Program Language:**

FORTRAN. **Running Time:** Depends on the degrees of freedom, M and N, and on whether the F ratio or the probability is being calculated. When calculating probability with $M = 1$ and $N = 1000$, about seven seconds are required.

A manual for this program will be published by the American Institute of Chemical Engineers, 25 West 45th Street, New York 36, N. Y., if sufficient interest develops. Reprinted from *Chem. Eng. Prog.* 1961, 57, 4, 88, where it was program 071.

An IBM 650 Program to Calculate Average Ridits. *Elaine P. Hoffman, University of Kentucky.* (**CPA 56**)

Description: Ridits are empirical transformations, related to the probability transformation, which are useful in reducing frequency data in a manner that allows direct probability statements to be made regarding differences between two or more frequency arrays. Bross developed ridit analysis for use with "borderland" variables, i.e., variables that fall somewhere between dichotomous classifications and refined measurement systems. Frequencies of response on ordered subjective scales (e.g., "minor", "moderate", "severe" or "reasonable", "possible", "improbable", "impossible") are readily amenable to ridit analysis.

Computer: The program is written for an IBM 650 with floating decimal device, index registers, and immediate access storage. Output is given directly by an IBM 407 on-line, but the program may be modified to give standard 10-8 punched card output. **Comments:** The program accepts data on individuals classified on two criteria, R and C—income group and rural or urban residence, for example. Frequency distributions are built up on a third variable S, the variable to be transformed, within the R and C classifications. The program computes the average ridits for each cell in the R by C table and the average ridits for row and column totals in the R by C table.

A description of the program complete with a listing and operator's instructions has been deposited as Document Number 6806 with the ADI Auxiliary Publications Project, Photoduplication Service, Library of Congress, Washington 25, D. C. Advance payment of $2.50 for photoprints, or $1.75 for 35mm microfilm should be made in ordering from: Chief, Photoduplication Service, Library of Congress.

Reference:

Bross, I. D. J. How to use ridit analysis. *Biometrics*, 14, 18–38.

Why level downward to our dullest perception always, and praise that as common sense? The commonest sense is the sense of men asleep, which they express by snoring. Sometimes we are inclined to class those who are once-and-a-half witted with the half-witted, because we appreciate only a third part of their wit.

HENRY DAVID THOREAU, *Walden*

ABSTRACTS OF CURRENT LITERATURE WITH INTERDISCIPLINARY IMPLICATIONS

Prepared by Members of the Staff of the Mental Health Research Institute

FROHMAN, C. E., TOURNEY, G., BECKETT, P. G. S., LEES, H., LATHAM, L. K., AND GOTTLIEB, J. S. **Biochemical identification of schizophrenia.** *AMA Arch. gen. Psychiat.*, 1961, 4, 404–412.

In recent years Dr. Frohman's group has been actively investigating a factor associated with the α-globulin fraction of plasma from schizophrenic subjects. This factor is reported to inhibit reoxidation of coenzyme I by red blood cells and thereby lead to an increased ratio of lactic to pyruvic acid. Furthermore, experiments with glucose labeled in the 1 and 6 positions indicate that this factor increases the amount of glucose degraded to pyruvate by the Meyerhoff-Embden scheme, and decreases the amount metabolized by pathways such as the hexosemonophosphate shunt. The factor responds to insulin stress by decreasing Meyerhoff-Embden metabolism. Red cells in the absence of this factor (i.e., incubated with plasma from normal subjects) show just the opposite pattern following insulin stress, increasing Meyerhoff-Embden metabolism.

In the present paper the authors have extended their observations to the schizophrenic population at the National Institutes of Health. Although extension of observations to this particular population has led to the quick demise of more than one biological test for schizophrenia, the results in this case appear rather encouraging although disagreement between the psychiatrists from the Lafayette Clinic and those at N.I.H. on the diagnosis of two subjects complicate the data.

The authors attempted to discriminate diagnostic population on a blind basis using as criteria the effects of appropriate plasma on chicken erythrocyte production of lactate and pyruvate and on the conversion of C_1 and C_6 labeled glucose to carbon dioxide before and after insulin stress. Of 11 subjects diagnosed as schizophrenic by the Lafayette psychiatrists, 41% were classified correctly and 59% were labeled questionable because of conflicting results on the two biological tests. All of 7 nonschizophrenic subjects were correctly classified. The diagnosis of the two contested subjects and the results obtained with them are somewhat unclear, but assuming the likely possibility that they were both nonschizophrenics, the figures using the N.I.H. diagnosis would be 78% correct for the nonschizophrenic population rather than 100%.

A similar blind discrimination with patients at the Lafayette Clinic using lactate/pyruvate ratio as the assay correctly identified 79% of 19 schizophrenic subjects and 84% of 13 nonschizophrenics. Discrimination using labeled glucose was at the near chance level (correct identification of 67% of the schizophrenic and 53% of the nonschizophrenic subjects).

Although studies on childhood schizophrenics did not confirm the diagnostic value of these procedures, the present study does strengthen the view that this procedure may be relevant to studies on schizophrenia. Whether this relevancy is related to the fundamental disease process or such factors as enduring effects of treatment of that process is still open to conjecture. While it would seem odd that such a fundamental biological process as energy metabolism would be involved in as subtle a process as schizophrenia, this study, as well as others in the literature, precludes dismissing this possibility and, indeed, may indicate a more complete search is in order. (*AY*)

ROBY, THORNTON B., AND LANZETTA, JOHN T. **A study of the "assembly effect" in small group performance.** *J. soc. Psychol.*, 1961, 53, 53–68.

This is an experimental investigation of the possibility that some combinations of

people may work together more effectively than other combinations of roughly the same total ability. Roby and Lanzetta made two major changes in the attack of previous investigators: (1) instead of a motor-skill task, i.e., spiral and ball, a cognitive task was used, and (2) a better opportunity to assess the potential contribution of each person was obtained.

The task was to produce a "target setting" of lights on three consoles, each having three lights. This was to be done by pushing buttons, one of which was on each console. Both the "target settings" and the button-light relationships were unknown to the subjects. Eight different maps, or target setting and button-light combinations, were used. Nine subjects were run individually, using one of the maps. On the basis of error scores (number of button pushings to target, from initial setting) these were divided into three ability levels. Nine groups were set up, each randomly composed of one individual from each ability level. No two individuals were in the same group together more than once. These groups were then run on the remaining seven maps, one person at each of the consoles. This arrangement made communication necessary for the solution of the problem.

Error scores and time scores were recorded. A tape recording of the conversations between the subjects during the solution of the problems was made.

An analysis of variance showed little evidence of statistical interaction effects between individuals. The variance in group scores that could be attributed to individuals could only partially be accounted for by the individual's ability as evidenced in individual performance. It appeared, however, that where the more proficient members controlled the activities of the group, the group tended to be more successful.

The authors suggest that group performance is a function of both individual characteristics that are relatively constant group-to-group, and those that are more complexly related to other individual or task characteristics.

This article should be of interest to small-group psychologists and to sociologists concerned with "structural" or "composition" effects. (*EB*)

DEUTSCH, MORTON, AND KRAUSS, ROBERT M. **The effect of threat upon interpersonal bargaining.** *J. abn. soc. Psychol.*, 1960, 61, 181–189.

This experiment was concerned with the effect of the availability of threat upon bargaining in a two-person game. Two assumptions are made about threat: (1) If there is a conflict of interests and a means of threatening the other person exists, there will be a tendency to use the threat in an attempt to force the other person to yield. This tendency will be stronger the more irreconcilable the conflict is perceived to be. (2) If threat is used to intimidate another, the threatened person will feel hostility toward the threatener and will tend to respond with counterthreats and/or increased resistance to yielding. "To allow one's self to be intimidated is to suffer a loss of social face and of self-esteem." The use of threat should strengthen the competitive interests of the bargainers; the availability of a means of threat should increase the difficulty of the bargainers' reaching agreement. It was hypothesized that a bargaining agreement is more likely to be reached when neither party can threaten the other than when one or both can threaten.

In this experiment, female telephone company personnel were asked to imagine that they were in charge of a trucking company carrying merchandise over a road to a destination. Each trip was worth 60 cents minus operating costs, calculated at 1 cent per second. The players start from separate points and go to separate destinations. At one point their paths cross; this is a one-lane road. An alternate route is available, but it is 56% longer. There is a gate at each end of the one-lane road, under the control of the closest player; this is the threat potential. In the bilateral threat condition, each of the two players, Acme and Bolt, controlled a gate; in the unilateral condition only Acme controlled a gate. In the third condition, neither player controlled a gate. The game was mechanized and

subjects could move their trucks forward or reverse by adding or subtracting numbers on a counter, determining their positions by reference to corresponding numbers on a "road map." Subjects did not know each other's position in this experiment except when they met head-on on the one-lane road. One subject then had to reverse before the other could go forward; panel lights indicated whether gates were open or closed. There was no direct transfer from one route to the other. Players had to back up to the start in order to change routes. When subjects had control of gates, they could close them only when they were travelling the main route; they could open their gates at any point in the game.

The instructions stressed an individualistic orientation. Subjects were given $4.00 in chips to start with. They were paid winnings and losses in chips after each trial when the net outcome of each subject was announced. Subjects were aware that no real money would change hands. Each pair played 20 trials; 16 pairs of subjects were used in each of the three conditions.

The best measure of the difficulty experienced by the bargainers in reaching an agreement was the sum of each pair's profits (or losses) on a given trial. The higher the joint payoffs, the shorter time it took to reach an agreement about sharing the one-lane road. It was easiest to reach agreement in the no-threat condition; it was most difficult or impossible in the bilateral threat condition. The joint payoffs tended to improve as the number of trials increased. In the unilateral condition, the gate controller, Acme, fared somewhat better than Bolt; Bolt, however, did better in the unilateral than in the bilateral condition.

The size of the absolute discrepancy between the two players indicates the amount of difficulty in predicting the other's actions. This discrepancy was smallest in the no-threat condition; it was initially relatively larger, then decreased more noticeably in the unilateral condition by the tenth trial, and was consistently smaller than the bilateral condition.

Evidently, most subjects were either egocentrically or competitively oriented to one another and felt that they would not be worse off using threats; the typical response is then counterthreat or increased resistance to being intimidated. Frequently efforts seem to be directed toward maintaining self-esteem. (*CJO*)

PARE, C. M., SANDLER, M., AND STACEY, R. S. **5-hydroxyindoles in mental deficiency.** *J. Neurol. Neurosurg. Psychiat.*, 1960, 23, 341–346.

Despite the considerable social, economic, and mental health problems associated with mental deficiency states, only a few chemically-oriented studies have been attempted in this area. In a previous investigation the authors demonstrated a decreased production of 5-hydroxytryptamine (5-HT) in phenylketonuric mental defectives (Pare, Sandler, and Stacey, *Lancet*, 1, 551, 1957). In the investigation reported here, 83 nonphenylketonuric mentally-defective patients of all ages were examined and compared to 68 control subjects. In most patients the serum 5-HT levels and the urinary 5-hydroxyindoleacetic (5-HIAA)/creatinine ratios were considerably raised. No significant differences were found between normal adults and children for either 5-HT or 5-HIAA.

In an attempt to explain the apparent causal relationship between 5-hydroxyindoles and deficiency states, a number of etiological factors and clinical features of patients were examined. No significant differences were found between patients whose deficiency was genetic in origin and those who suffered damage during intrauterine life or at birth. Also, no relationship was observed between the degree of deficiency—determined by a crude rating method since the usual intelligence tests are necessarily inaccurate—and levels of 5-HT. (*NM*)

MATURANA, H. R., LETTWIN, J. Y., MCCULLOCH, W. S., AND PITTS, W. H. **Anatomy and physiology of vision in the frog (Rana Pipiens).** *J. gen. Physiol.*, 1960, 43, 129–175.

By microelectrode techniques, the authors were able to record single-fiber responses to

visual stimuli in the optic nerves of unanesthetized frogs. They found that the fibers fall into five classes: Class 1, sustained edge detection; Class 2, convex edge detection; Class 3, changing contrast detection; Class 4, dimming detection; Class 5, darkness detection. The cells of the first four classes respond to moving objects, and the cells of the fifth class respond to general illumination. The authors found that the retina does not transmit point-to-point information about the object, but transmits information on the qualitative aspects of the object (curvature, edges, contrast, etc.) (*BP*)

SIEGEL, BERNARD, AND BEALS, ALAN. **Conflict and factionalist dispute.** *J. R. anthrop. Inst.*, 1960, 90, 107–117.

The focus of this article is a specific type of conflict, namely factionalism. Factionalism refers to an overt, unresolved dispute over means rather than ends which interferes with the achievement of group goals. A group is conceived to be "a partially self-regulating system," and thus is affected by both the interaction within the system and the interaction of the system with the supersystem. Factionalism is the product of external stress and internal strains. The authors conceive of stress as being finite and not necessarily perceived by the group members. It has six dimensions: covertness or ease of perception, randomness or regularity with which it appears, curtailment or extent to which it impedes normal activity, selectivity or extent to which subunits are differentially affected, duration, and complexity. Strain refers to "potential cleavages inherent in key-role and group alignments." Factionalism is most likely to occur when the existing group cleavages are accentuated by external forces influencing the group in a selective and covert manner

This article is particularly relevant to a study of the nature of social change. Within developing nations, the agreed-upon end is modernization; the internal unrest which often accompanies modernization is a result of a dispute over the means which should be utilized in order to attain this end. Contact with nations which have successfully passed through a period of modernization introduces internal strains. Thus, Western education, the mass media, and other intrusions upon the native culture introduce new criteria and new channels for advancement and prestige, and create new reference groups within the population and especially within the elite and counterelite. The resulting conflict detracts greatly from the co-operative effort essential to mould a state in a new or partially new form. (*ECS*)

Any man may speak truthfully; but to speak methodically, with wisdom and talent, is given to few.

MICHEL EYQUEM DE MONTAIGNE, *Of the Art of Conversing*

ABOUT THE AUTHORS

B. K. Bagchi ("Studies of Autonomic Functions in Practitioners of Yoga in India") is currently professor of electroencephalography at The University of Michigan. He received the M.A. degree from Calcutta University, and the Ph.D. degree from the University of Iowa. Dr. Bagchi writes, "I have been theoretically and practically interested in Yoga since my boyhood in India, but except for some preliminary relaxation studies I had never had an opportunity to investigate Yoga from an experimental scientific angle. So when Wenger proposed to go to India in 1957 I decided to join him. We met hundreds of people there—from sweepers, to swamis, yogis, interested scientists, cranks, heads of religious organizations, heads of government, including the Planning Minister, the Vice-President of the Indian Republic, the Health Minister, High Court Justices, etc. They were all stimulating. We heard stories that could fill a book. I hope some of our recommendations to the India Government are accepted and the door kept open for further scientific work on the subject."

Roland R. Bonato ("A General Cross-Classification Program for Digital Computers") graduated with a B.A. degree from Western Reserve University, attended Los Angeles State College, and obtained his M.A. and Ph.D. degrees from the University of Connecticut. He is presently a research psychologist at George Washington University. His current research interests lie in the fields of driver behavior, opinion and attitudes, and data processing methods.

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Robert J. Kleiner ("Multiple Group Membership and Schizophrenia") received his B.S. degree from the City College of New York, his M.A. degree from Western Reserve University, and his Ph.D. degree from the University of Pennsylvania. He is currently Director of the Research and Statistics Unit, Eastern Mental Health Center, Pennsylvania Department of Public Welfare. He has published many papers in national and international journals presenting the empirical results of studies in the area of his interests. He has also participated in several symposiums at national and international meetings and conferences. At the present time Dr. Kleiner is engaged in several research projects concerned with social class, mobility striving, and mental illness.

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Anatol Rapoport ("A Study of a Large Sociogram"), professor of mathematical biology and senior research mathematician at the Mental Health Research Institute, The University of Michigan, is a member of the editorial board of *Behavioral Science*.

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J. David Singer ("The Relevance of the Behavioral Sciences to the Study of International Relations" and "Primary Groups and the Political System: Simulation and Reality") has recently joined the Mental Health Research Institute of The University of Michigan as associate research political scientist. Formerly in the University's Department of Political Science, he crossed the campus via a six-month "tour of duty" as International Relations Consultant at the Naval War College. He is the author of *Financing International Organization: The U. S. Budget Process* and the forthcoming *Deterrence, Arms Control, and Disarmament; Toward a Synthesis in National Security Policy*, as well as a number of articles in professional journals. He is currently working on the problem of quantifying key international relations variables, and is completing a selective cataloging of the behavioral science literature bearing on this subject, tentatively entitled *State Behavior in World Politics: Readings from the Social Sciences*. Dr. Singer received the A.B. degree from Duke University, and the Ph.D. degree from New York University.

Jacob Tuckman ("Multiple Group Membership and Schizophrenia") received the Ph.D. degree from Columbia University. He is currently Chief of the Section of Psychological Services, Education and Standards, Division of Mental Health, Philadelphia Department of Public Health. Dr. Tuckman is the author of more than seventy publications, including *Retirement and the Industrial Worker*, with Irwin Lorge.

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As director of the Family Injury Survey of the Connecticut State Department of Health, Dr. Waxman has conducted an accident research study supported by the National Institutes of Health. After four years of research, this project is nearing completion and results are presently being prepared for publication. Dr. Waxman has published several articles in professional journals and, with A. Tutles, has written *Human Accidents: A Study of Their Causes and Prevalence*, now in press.

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With Volume 6, Number 3 of *Behavioral Science* Mrs. Joan Cartwright Barth, whose duties have been those of editorial assistant, has left the journal. The Board of Editors has received her resignation with deep regret, and wishes publicly to express its thanks and appreciation for her invaluable services during the past three and one-half years.

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and

THE JOURNAL OF CONFLICT RESOLUTION

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BEHAVIORAL SCIENCE
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January 1962

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THE JOURNAL OF CONFLICT RESOLUTION
Volume 6, Number 1
March 1962

THEORETICAL CONSIDERATIONS OF GAME THEORY TO INTERNATIONAL CONFLICT

The contents of this issue will include a number of papers from a symposium to be held at Princeton University, sponsored by the Carnegie Foundation under the auspices of the Institute for Defense Analysis. The theme of the symposium is "Applications of Game Theory to Negotiation," and the conference directors are Harold Kuhn and R. Duncan Luce. In addition to these papers, contributions to the issue will be made by Karl Deutsch, Ward Edwards, and others. A complete listing of titles and authors will appear in the January, 1962 issue of *Behavioral Science.*

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